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*THE ORIGIN OF HYPOTHESES, ILLUSTRATED BY THE DISCUSSION OF A TOPOGRAPHIC PROBLEM.**

AN important part—in some respects the most important part—of the work of science is the explanation of the facts of Nature. The process through which natural phenomena are explained is called the 'method of hypotheses,' and though it is familiar to most of my audience I shall nevertheless describe it briefly for the purpose of directing special attention to one of its factors.

The hypothesis has been called a 'scientific guess,' and unless the title 'guess' carries with it something of disrespect it is not inappropriate. When the investigator, having under consideration a fact or group of facts whose origin or cause is unknown, seeks to discover their origin, his first step is to make a guess. In other words, he frames a hypothesis or invents a tentative theory. Then he proceeds to test the hypothesis, and in planning a test he reasons in this way: If the phenomenon was really produced in the hypothetic manner, then it should possess, in addition to the features already observed, certain other specific features, and the discovery of these will serve to verify the hypothesis. Resuming

* Annual Address of the President of the Geological Society of Washington; read December 11, 1895, to the Scientific Societies of Washington. By special arrangement, through the Joint Commission of those societies, this number of SCIENCE is mailed to all members.

its examination, he searches for these particular features. If they are found the theory is supported; and in case the features thus predicted and discovered are numerous and varied, the theory is accepted as satisfactory. But if the reëxamination reveals features inconsistent with the tentative theory, the theory is thereby discredited, and the investigator proceeds to frame and test a new one. Thus, by a series of trials, inadequate explanations are one by one set aside, and eventually an explanation is discovered which satisfies all requirements.

When the subject of study is one of wide interest it usually happens that several investigators coöperate in the invention and testing of hypotheses. Often each investigator will originate a hypothesis, and a series of rigorous tests will be applied through the endeavor of each one to establish his own by overthrowing all others. The different theories are rivals competing for ascendancy, and their authors are also rivals, ambitious for the credit of discovery. The personal factor thus introduced tends to bias the judgment and is to that extent unfavorable to the progress of science; but the conflict of theories, leading, as it eventually must, to the survival of the fittest, is advantageous. Fortunately there is a mode of using hypotheses which regulates the personal factor without restricting the competition of theories, and this has found favor with the greatest investigators. It has recently been formulated and ably advocated by our fellow-member, Prof. T. C. Chamberlin, who calls it the 'method of multiple hypotheses.'*

In the application of this method the student of a group of phenomena, instead of inventing and testing hypotheses one at a time, devises at an early stage as many as possible, and then, treating them as rival claimants, assigns to himself the rôle of judge. Returning to the study of nature, he seeks for special features which cannot consist with all the hypotheses, and may therefore serve to discriminate among them. Thus by a series of crucial tests he eliminates one after another of the tentative theories until but a single one remains, and he then proceeds to apply such tests as he may to the survivor.

In these methods of work, whether theories are examined successively or simultaneously, there are two steps involving the initiative of the investigator; he invents hypotheses and he invents tests for them. It is to the intellectual character of these inventions that your attention is invited.

The mental process by which hypotheses are suggested is obscure. Ordinarily they flash into consciousness without premonition, and it would be easy to ascribe them to a mysterious intuition or creative faculty; but this would contravene one of the broadest generalizations of modern psychology. Just as in the domain of matter nothing is created from nothing, just as in the domain of life there is no spontaneous generation, so in the domain of mind there are no ideas which do not owe their existence to antecedent ideas which stand in the relation of parent to child. It is only because our mental processes are largely conducted outside the field of consciousness that the lineage of ideas is difficult to trace.

To explain the origin of hypotheses I have a hypothesis to present,—not, indeed, as original, for it has been at least tacitly assumed by various writers on scientific method, but rather as worthy of more general attention and recognition. It is that hypotheses are always suggested through analogy. The unexplained phenomenon on which the student fixes his attention resembles in some of its features another phenomenon of which the explanation is known. Analogic reasoning suggests that the desired explanation is similar in char-

*The Method of Multiple Working Hypotheses, Science (1st series), Vol. XV. (1890) pp. 92–96.

acter to the known, and this suggestion constitutes the production of a hypothesis.

To test this hypothesis of hypotheses I have for some years endeavored to analyze the methods employed by myself and some of my associates in geologic research, and this study has proved so interesting in connection with the investigation of a peculiar crater in Arizona, that I shall devote the remainder of my hour to an outline of that investigation.

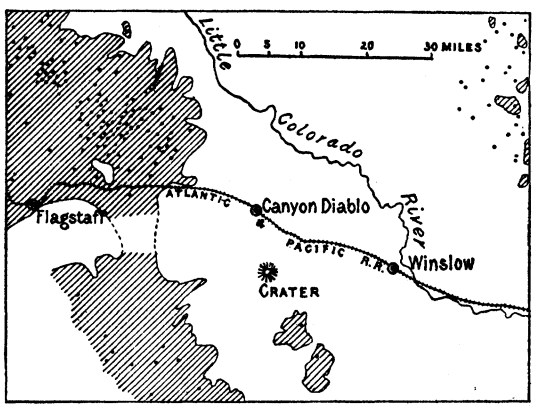

FIG. 1.—Map of part of northern Arizona. The shaded areas are covered by volcanic rocks. Dots mark ancient volcanic vents.

In northeastern Arizona there is an arid plain beneath whose scanty soil are level beds of limestone. At one point the plain is interrupted by a bowl-shaped or saucer-shaped hollow, a few thousand feet broad and a few hundred feet deep; and about this hollow is an approximately circular rim rising one or two hundred feet above the surface of the plain (Plate 1, Figs. 2 and 3). In other words, there is a crater; but the crater differs from the ordinary volcanic structure of that name in that it contains no volcanic rock. The circling sides of the bowl show limestone and sandstone, and the rim is wholly composed of these materials. On the slopes of this crater and on the plain round about many pieces of iron have been found, not iron ore, but the metal itself, and this substance is foreign to the limestone of the plain and to all other formations of the region.* The features of the locality thus include three things of unusual character and requiring explanation: First, the crater composed of non-volcanic rock; second, the scattered iron masses; third, the association of crater and iron. To account for these phenomena a number of theories have been suggested.

In the year 1886 a company of shepherds encamped on the slopes of the crater and pastured their sheep on the surrounding plain. Mathias Armijo, one of their number, found a piece of iron, and, deceived by its lustrous surface, supposed it to be silver. The mistake was quickly corrected by his fellows, but his discovery excited their interest, and other pieces of iron were soon found. The curiosity of the shepherds was aroused also by the crater, and they invented a theory which is admirable for its simplicity: The crater was produced by an explosion, the material of the rim being thrown out from the central cavity, and the iron was thrown out from the same cavity at the same time. You will observe that this theory is comprehensive. It accounts for the crater, the iron, and the association of the two. As I have never met these first students of the phenomena I have had no opportunity to make inquiry as to the origin of their theory; but its close relation to the theories of geologic disturbance which are current in mining districts suggests that it also sprung from the familiar process of blasting. As the firing of a blast opens a cavity and heaps dislocated rock masses in an irregular way, the unlearned miner finds in natural blasting an easy explanation of hollows and uplifts.

Four years later a man by the name of Craft saw in the iron a possibility of profit. Setting up a heap of stone to mark the spot,

* The crater is locally known as Coon Mountain, or Coon Butte. The iron is known to literature as the Canyon Diablo fall of meteorites.

he located a mining claim; and going to the city of Albuquerque, he announced that he had a vein of pure iron 40 yards wide and two miles long, and offered to sell his property to a railway company. The samples he submitted were examined by an assayer, and the officers of the company gave consideration to his proposal, agreeing to send a representative to examine the property. The negotiation was not concluded, because Mr. Craft, having borrowed money on the strength of his great expectations, mysteriously disappeared, but the incident served to give information of the locality to a scientific observer. The assayer forwarded a piece of the iron to the late Dr. A. E. Foote, the mineralogist, who visited the place, collected a quantity of the iron and examined the crater. In the summer of 1891 he communicated his observations to the American Association for the Advancement of Science,* which that year was the guest of the scientific societies of Washington, and his paper aroused much interest. For the crater of non-volcanic rock he offered no explanation, but the iron he pronounced of celestial origin—a shower of fallen meteors. It has long been known that many of the bodies which reach the earth from outer space are composed of iron, and that such iron is of peculiar character, having a certain crystalline structure, being alloyed with nickel, and including nodules of certain substances which are not found in any other association. So Doctor Foote, in characterizing the iron as meteoric, merely referred it to a well-established class. His explanation was not tentative, but final, and has not been called in question by any subsequent investigator.

In the discussion following the reading of his paper a new hypothesis was proposed, and as this was offered by myself I can trace its origin with comparative confidence. The crust of the earth is not equally dense at all points, but some parts are heavier than others. Not only are there variations from hill to hill and from formation to formation, but the continents are in general composed of lighter materials than the ocean beds, and one side of the sphere is so much heavier than the other that its attraction pulls most of the water away from the other side. Among the various theories that have been proposed for the origin of the planet there is one which ascribes it to the falling together under mutual attraction of many smaller celestial bodies, and it has been suggested that the variations in the crust may represent original differences of the concurrent masses. Speculating on such lines I had asked myself what would result if another small star should now be added to the earth, and one of the consequences which had occurred to me was the formation of a crater, the suggestion springing from the many familiar instances of craters formed by collision. A raindrop falling on soft ooze produces a miniature crater; so does a pebble thrown into a pool of pasty mud. A larger crater is made when a steel projectile is fired against steel armor plate; and analogy easily bridged the interval from the cannon ball to the asteroid. So when Dr. Foote described a limestone crater in association with iron masses from outer space, it at once occurred to me that the theme of my speculation might here find its realization. The suggested explanation assumes that the shower of falling iron masses included one larger than the rest, and that this greater mass, by the violence of its collision, produced the crater. Here again you will observe that a single theory explains the crater, the iron and their association.

The thought of examining the scar produced on the earth by the collision of a star

* A new locality for meteoric iron with a preliminary notice of the discovery of diamonds in the iron. Proc. Am. Ass. Adv. Science, Vol. 40, pp. 279–283.

was so attractive that I desired to visit the crater, but as that was not immediately practicable I arranged to have it visited by one of my colleagues. A few months later Mr. Willard D. Johnson spent several days at the locality, making a sketch map and describing the various features. When he reached the rim of the crater he found it to consist chiefly of limestone strata inclined outward, and his first thought was that the rim might be the remnant of the dome of strata over a laccolite. The laccolite is a peculiar volcanic product. The molten lavas which make volcanoes rise from deep sources through cracks or passages among the rocks and flow out over the surface of the land; but sometimes rising lavas fail to reach the surface, and accumulate at lower levels, opening for themselves bubble-shaped chambers over which the strata are arched. In the dome-like structures thus produced the rocks dip outward in all directions from a central region, and this outward dip was the feature which, through analogy, suggested to Mr. Johnson a laccolitic origin. His first idea, however, was not long retained, for examining the walls and bottom of the crater he found no trace of the igneous rocks of which laccolites are composed, and the theory afforded no aid in accounting for the hollow. He therefore dismissed it and sought another. He may have considered several others, but the only one placed on record is an explosion theory. In some way, probably by volcanic heat, a body of steam was produced at a depth of some hundreds or thousands of feet, and the explosion of this steam produced the crater. The fall of iron was independent, and the association of the two occurrences in the same locality is accidental.* As Mr. Johnson is at once a civil engineer and a student of geology and geography, he had at command as basis for analogic reasoning the explosive phenomena associated with the arts and also those which belong to the history of volcanoes, and we may assume that these suggested his theory.

Mr. Johnson's account of the crater was much fuller than Dr. Foote's, but instead of satisfying my curiosity tended rather to whet it, and I availed myself of the first opportunity to make a personal visit. Four hypotheses had now been made, but only two survived. The theory of the shepherds, deriving the iron from the cavity of the crater, was disproved by Dr. Foote's determination of the meteoric character of the iron. The laccolitic theory had been promptly set aside by Mr. Johnson. There remained the theory of a star's collision and the theory of a steam explosion. If my visit was to aid in the determination of the problem of cause it must gather the data which would discriminate between these two theories, and an attempt was accordingly made to devise crucial tests. If the crater was produced by the collision and penetration of a stellar body that body now lay beneath the bowl, but not so if the crater resulted from explosion. Any observation which would determine the presence or absence of a buried star might therefore serve as a crucial test. Direct exploration by means of a shaft or drill hole could not be undertaken on account of the expense, but two indirect methods seemed feasible.

If the crater were produced by explosion the material contained in the rim, being identical with that removed from the hollow, is of equal amount; but if a star entered the hole the hole was partly filled thereby, and the remaining hollow must be less in volume than the rim. The presence or absence of the star might therefore be tested by measuring the cubic contents of the hollow and of the rim and comparing the two. Of the intellectual origin of this

* Mr. Johnson's discussion of the problem was communicated to me in a personal letter. G. K. G.

test perhaps the most that can be said is that it is a test by quantities, and that the experienced investigator, having previously found relations of quantity the most satisfactory criteria, habitually employs them whenever the circumstances permit.

Again it occurred to me that the stellar body would presumptively be composed, like the smaller masses round about, of iron, and that its presence or absence might, therefore, be determined by means of the magnetic needle. If it were absent the compass would point in the same direction, whatever its position with reference to the crater, whether within or without, on one side or the other, near by or miles away; but if a mass of iron large enough to produce the crater lay beneath it its attraction would pull the needle one way or the other, producing local variations. Doubtless the suggestion of this test came from knowledge of the methods employed in searching for magnetic iron ore in northern Michigan, where the prospector carries the dip needle to and fro through the forest, and by means of its changes of direction determines the position and extent of bodies of ore.

As an equipment for these measurements I provided myself with the instruments necessary to make an accurate topographic map, and obtained, through the courtesy of the Coast and Geodetic Survey, a full set of instruments for the observation of terrestrial magnetism. I was so fortunate, also, as to secure the coöperation of an expert magnetic observer, Mr. Marcus Baker, of the Geological Survey, and together we set out for Arizona.

At this time it seemed to me that the presumption was in favor of the theory ascribing the crater to a falling star, because that theory explained, while its rival did not, the close association of the crater with the shower of celestial iron. So far as we know, a falling meteor is just as likely to reach any one spot on the earth's surface as any other, and it is, therefore, entirely possible that the coincidence of the meteoric locality with the locality of the crater has no special significance; but if the two phenomena are not connected by a causal relation, it is no more probable that the crater should coincide in place with one of the 165 meteoric falls recorded within the bounds of the United States than that it should occupy any other spot of our broad domain. A rough estimate shows the probability of non-coincidence to be at least 800 times as great as the probability of coincidence. This by no means warrants the conclusion that an explanation ascribing a causal relation is 800 times as probable as one ascribing fortuitous coincidence, but it legitimately inclines the mind toward causality in the absence of more direct and authoritative evidence.

This point is illustrated by the investigation of the peculiar sky colors observed twelve years ago. Considering the phenomenon of coloration in its entirety—character, distribution and duration—it was not merely rare, it was unique. In the same year a tremendous volcanic explosion occurred in the Straits of Sunda, and that also was unique in intensity. The coincidence of the two, which in this case was a matter of time rather than place, led to the belief that the one was caused by the other, and this belief was held by many men of science before an adequate explanation of the mode of causation had been suggested.

So when Mr. Baker and I started for the crater it seemed rather probable than otherwise that we should find a local deflection of the magnetic needle, and that we should find the material of the rim more than sufficient to fill the hollow it surrounds.

Before our journey was ended another explanation suggested itself. Mr. Johnson had described the crater as not truly circular but somewhat oval, the longer diameter lying east and west. He noted also that

ILLUSTRATIONS TO ARTICLE BY G. K. GILBERT ON HYPOTHESES.

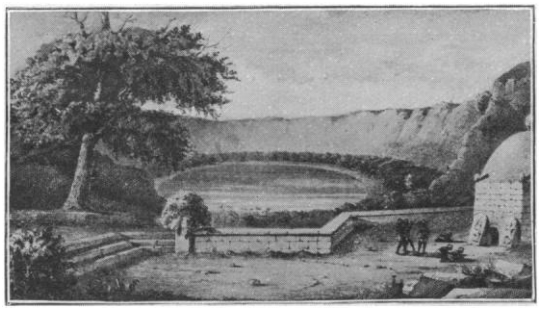

FIG. 1.—Lonar Lake, India, occupying an explosion crater. From Newbold's *Summary of the Geology of Southern India*, Jour. Roy. Asiatic Soc., vol. 9, p. 40. London, 1848.

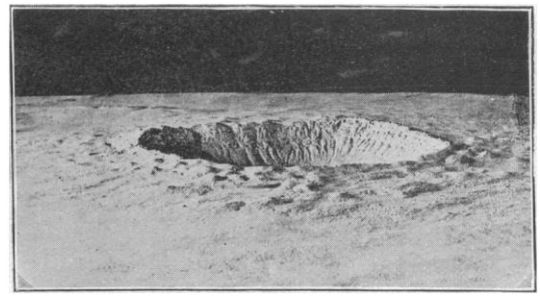

FIG. 2.—The limestone crater of Arizona, Coon Butte, as seen from the south. Photograph of a model by Mr. Victor Mindeleff.

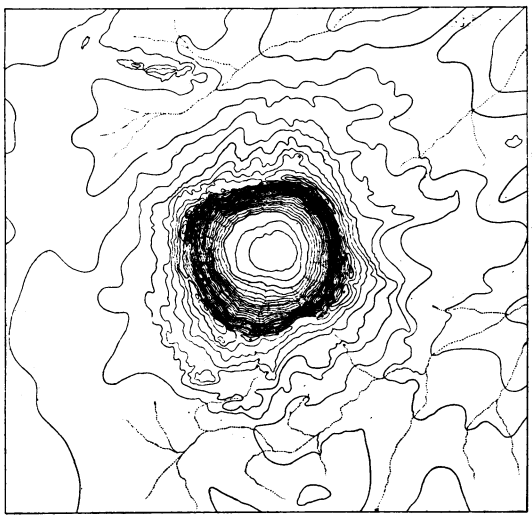

FIG. 3.—Contour map of Coon Butte. The vertical distance from contour to contour is ten feet. Lines of drainage are dotted.

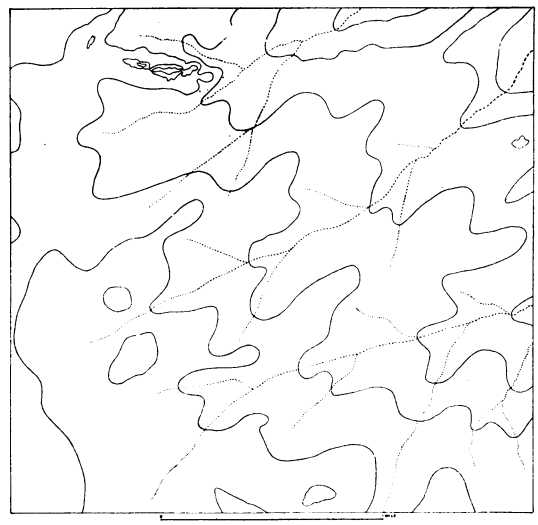

FIG. 4.—Restoration of the site of Coon Butte before the formation of the crater. Contour interval, ten feet; lines of drainage dotted. Compare with Fig. 3.

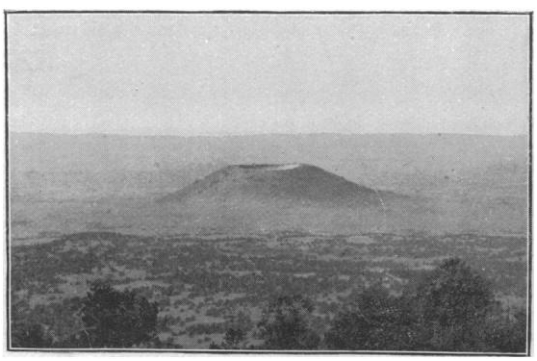

FIG. 5.—Volcanic cinder-cone, with crater, north of San Francisco Mountain, Arizona. The position of the crater, at top of the hill, is characteristic of most volcanoes. Compare Fig, 2, where the crater lies chiefly below the level of the plain.

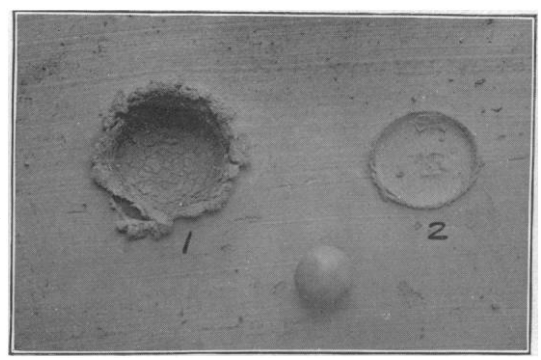

FIG. 6.—Craters made by throwing clay balls at a clay target. A ball of the same size is shown. 1 shows the effect of high velocity, 2 of low.

ILLUSTRATIONS TO ARTICLE BY G. K. GILBERT ON HYPOTHESES.

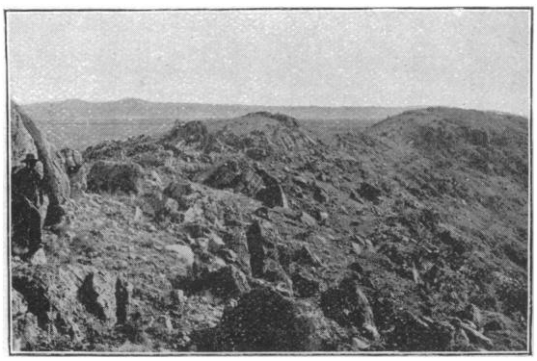

FIG. 1.—Rim of Coon Butte, with part of inner face.

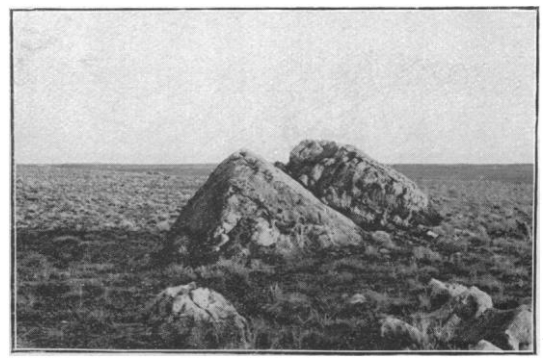

FIG. 2.—Block of limestone on outer slope of Coon Butte, one-half mile from rim.

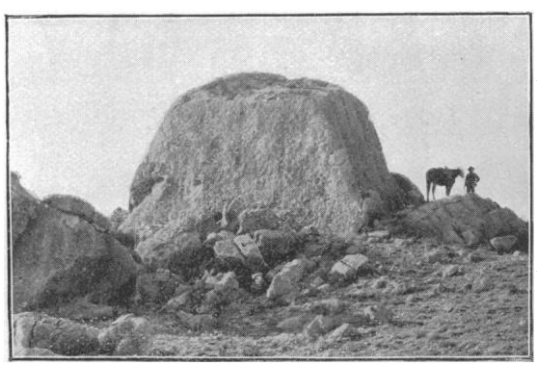

FIG. 3.—Largest block of limestone on rim of Coon Butte. Diameter, 60 feet.

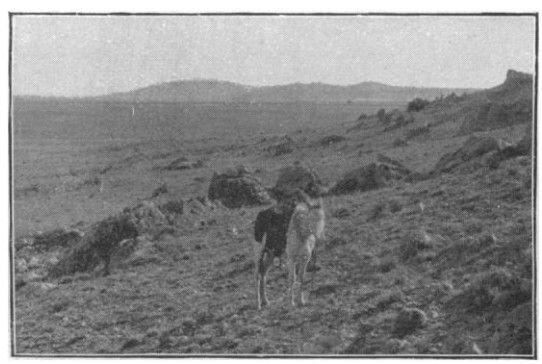

FIG. 4.—Outer slope of Coon Butte.

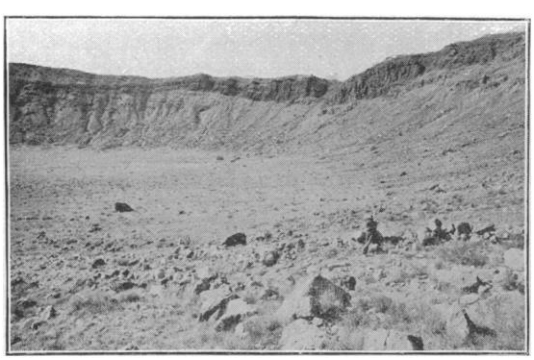

FIG. 5.—Interior of Coon Butte, as seen from the talus on one side. The cliff below the rim is of limestone.

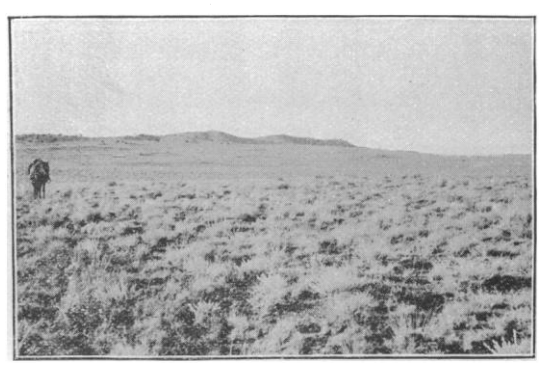

FIG. 6.—Exterior of Coon Butte, as seen from the surrounding plain.

the rim was bulkier on the east side than on the west, and that nearly all the iron had been found east of the crater. The new explanation was that a star, falling obliquely from the western sky, struck the earth and bounded off, finally coming to rest at some point farther east. The idea was of course derived from the ricochet of projectiles; I had seen the mark left by a rifle ball where it rebounded from a plowed field. This explanation could be tested by a simple examination of the topographic form; and it may be as well to anticipate here the order of the narrative, and say that the form of the crater was found to be quite inconsistent with the ricochet hypothesis. The difference of the two diameters is quite small; the eastern rim is but little more massive than the western; and the dislocation of the rocks in the western rim is of such character that it could not have been produced by a body descending obliquely toward the east.

Arriving at the crater we spent two weeks in topographic and magnetic surveys and the study of local details. The diameter of the bowl, measured from rim to rim, is about three-fourths of a mile. Its depth below the rim is from 550 to 600 feet; below the plain, 400 feet, the rim being 150 to 200 feet high. The rim is in part composed of limestone strata like those which underlie the plain, but turned up, so as to incline steeply away from the hollow on all sides. On the inclined strata rests a mantle of loose fragments which are in part of limestone and in part of sandstone. The limestone masses are fragments of the formation occurring just beneath, and the sandstone masses are fragments of a formation which underlies the limestone formation. Most of the masses are of moderate size, but others are large, the limestone reaching a diameter of 60 feet, and the sandstone about 100 feet. (Plate 2. Figs. 1–4.). They are irregularly mingled, one material predominating in one tract and the other in another. The limestone is the more conspicious because withstanding better the attacks of the weather. In fact the larger blocks of sandstone have been so far washed away that they do not project above the surface. From the crest of the rim outward this loose material occupies the surface for an average distance of half a mile, being characterized by rolling or hummocky topography. At greater distances it is thinly spread and the constituent blocks are small. At one mile it is represented only by scattered fragments, but these continue with diminishing frequency to a distance of three and a half miles.

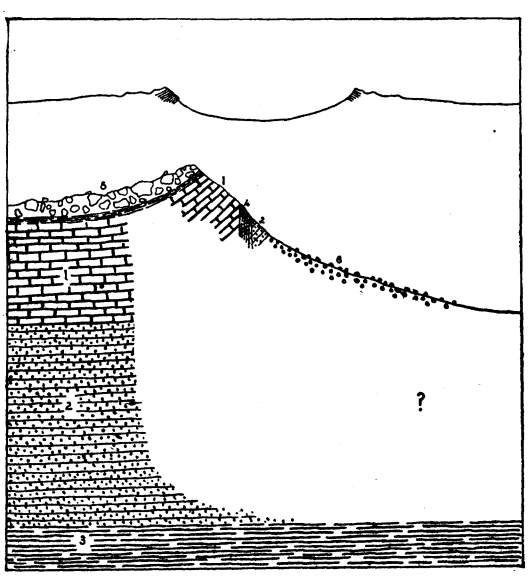

FIG. 2.—The upper diagram is profile across the crater; the lower, a cross-section of the rim. 1, limestone. 2, sandstone. 3, red shale. 4, crushed rock. 5, loose blocks of limestone and sandstone. 6, talus of debris fallen from 1 and 2 above.

Inside the rim the edges of limestone strata occupy the slope for a space of 150 to 250 feet. They are succeeded in several places by sandstone strata, but the sandstone does not hold its original relation to the limestone; it is separated by a vertical zone of crushed rock, and there is other evidence that it has been faulted upward.

The lower slopes are occupied by fragments of limestone and sandstone with an arrangement showing that they have fallen from the cliff above so as to constitute a talus of the ordinary type, and the central tract is composed of fine material of the same kind. Whatever may have been the original shape of the pit, its present form has resulted from subsequent modification under the action of rain and frost.

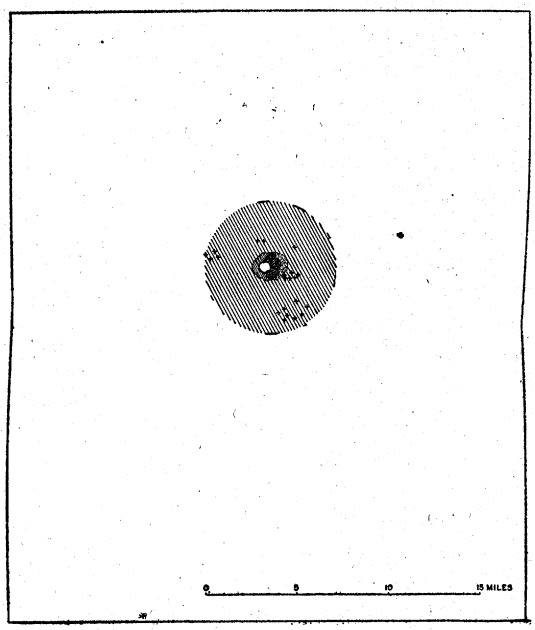

Fig. 3.—Distribution Chart. The inner line is the rim of crater. In the inner shaded area the loose debris has a depth of more than one foot. In the outer shaded area are scattered blocks; where the area is bounded by a line its limit was surveyed. The chief district of small iron masses is shown by dots. Large iron masses are indicated by crosses. The distribution of the iron is chiefly on the authority of Mr. F. W. Volz, of Canyon Diablo, A. T.

No iron has been found within the crater, but a great number of fragments were obtained from the outer slopes where they rested on the mantle of loose blocks. Many others were obtained from the plain within the region of scattered debris, and others, though a smaller number, from the outer plain. One large piece was discovered eight miles east of the crater, or almost twice as distant as any fragments of the ejected limestone. Another was long ago discovered twenty miles to the southward, but what became of it is not known, and it has not been definitely identified as a member of the same meteoric shower. Most of the masses are small. There have been found more than one thousand, possibly more than two thousand, pieces weighing less than an ounce; others weigh a few ounces or a few pounds; forty or fifty exceed one hundred pounds, and two exceed one thousand pounds. The total weight of all finds is probably ten tons. At the time of my visit I was told that all had been discovered east of a north and south line passing through the middle of the crater, but this may have been an accident of the method of search, for more recently six large ones have been reported from points west of that line.

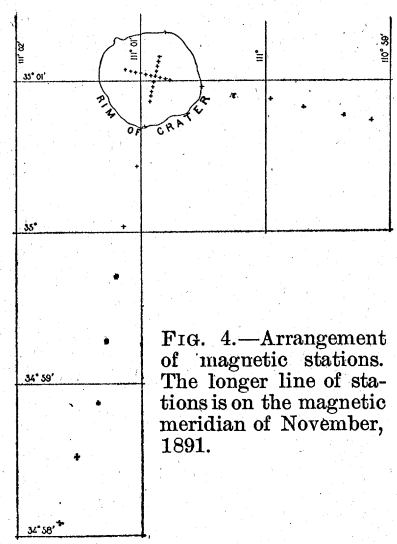

Fig. 4.—Arrangement of magnetic stations. The longer line of stations is on the magnetic meridian of November, 1891.

The magnetic survey by Mr. Baker included the selection and mapping of a system of stations, and the observation at each of the three magnetic elements: the horizontal component of direction, or the compass bearing; the vertical component

of direction, or the inclination of the dip needle; and the intensity of the magnetic force. Two lines of stations, at right angles to each other, were carried across the crater, and one of these lines was extended to a distance of three and a half miles on the plain. When the results were tabulated and compared, the magnetism was found to be constant in direction and intensity at all the stations, the deviations from uniformity being not greater than the unavoidable errors of observation. So if the crater contains a mass of iron its attraction is too feeble to be detected by the instruments employed. That we might learn the precise meaning of this result, the delicacy of the instruments was afterward tested at the Washington Navy Yard, by observing their behavior when placed in certain definite relations to a group of iron cannon whose weight was known, and the following conclusions were reached: If a mass of iron equivalent to a sphere 1500 feet in diameter is buried beneath the crater it must lie at least 50 miles below the surface; if a mass 500 feet in diameter lies there its depth is not less than 10 miles. So the theory of a great iron meteor is negatived by the magnetic results, unless we may suppose either that the meteor was quite small as compared to the diameter of the crater, or that it penetrated to a very great depth.

The topographic survey was executed with such detail as to warrant the drawing of contour lines for each ten feet of height. (Plate 1, Fig. 3.) During its progress the configuration of the surrounding country was carefully studied, and its general plan was found to be so simple and regular that the original contours before the creation of the crater could be restored without great liability of error. (Plate 1, Fig. 4.) Such restoration was made, and with its aid two quantities were afterwards computed: first, the cubic contents of the rim so far as it projects above the ancient surface; second, the cubic contents of the hollow so far as it lies below the ancient surface. The two volumes were compared with each other and also with the volume of a spherical projectile estimated as competent to produce the crater. From experiments with balls of clay fired against a target of the same material it seems probable that a crater 4,000 feet in diameter might be produced by a swift-moving meteor with a diameter of 1,500 feet. (Plate 1, Fig. 6.) It seems possible, though not probable, that it could be made by a mass 750 feet in diameter. The volume of the greater assumed projectile is 60 million cubic yards; the volume of the lesser, 7½ million yards. The magnitude of the hollow was found to be 82 million yards, and the magnitude of the rim was also found to be 82 million yards. It, therefore, appears that if the rim were to be dug away down to the level of the ancient plain, and the material tightly packed within the hollow of the crater, it would suffice to precisely fill that hollow and restore the ancient plain. The excess of matter required by the theory of a buried star was not found.

Thus each of the two experiments whose testimony had been invoked declared against the theory of a colliding meteor; and the expectation founded on the high improbability of fortuitous coincidence nevertheless failed of realization.

Attention being now directed to the only surviving theory, that of steam explosion, all the various features discovered in the local study were considered with reference to it. To describe and discuss them on this occasion would lead too far from our subject, and they may be passed by with the remark that, while not all are as yet fully understood, they seem not to oppose the theory.

For the sake of applying another quantitative test, an attempt was made to ascertain whether the energy which could be developed by heating the water contained in

the sandstone formation would be sufficient, when the overlying strata gave way, to hurl their fragments out upon the sursounding plain. As the data were quite indefinite the computation could result only in a rough approximation, and there is no need to weary you with its details, but it served to show that the assumed cause was of the same order of magnitude as the result accomplished. The idea of applying such a test needs no specific explanation, because quantitative tests of this particular type are among the most familiar resources of investigation. Whenever a tentative theory involves the application of force or the expenditure of energy the investigator (or his critic) habitually asks whether the assumed cause affords a sufficient amount of force or of energy.

Practically the same conclusion was reached in a more satisfactory way by studying the accounts of other natural explosions where steam was the agent. At several epochs in its history the top of Mount Vesuvius has been torn away by a sudden convulsion. In Java the summit of Mount Tomboro was blown away, with the production of a great crater which now contains a lake, and a similar catastrope occurred on the slope of Mount Pepandajáη. The great explosion of Krakatoa, in 1883, demolished several volcanic islands and created others, reconstructing the topography of a district in the Straits of Sunda. On July 15, 1888, a great opening was torn in the Japanese mountain Kobandai, the summit and part of one side being removed. The last mentioned instance is the most available for comparison because the agency of steam distinctly appeared, and because the history of the event has been admirably reported by two Japanese geologists, Profs. Sekiya and Kikuchi, of Tokio. *

* The Eruption of Bandai-san. Trans. Seismological Soc. of Japan, Vol. XIII., (1890), pp. 139–222. 9 plates.

There were in this case about twenty explosions, all occurring within the space of one or two minutes. A cloud of rock fragments ascended to a height of 4,000 feet. The greater number, moving obliquely away from the mountain side, fell upon its lower slope, down which they rolled as an avalanche for a distance of five miles, overwhelming several villages and transforming a fertile plain into a rocky desert. In other directions fell showers of stones, and a cloud of dust descending more slowly. The resulting crater, less regular in form than the subject of our study, was nineteen times as capacious, and from its bottom fierce jets of steam issued for weeks and even months. Kobandai is a volcanic peak, and although it had been quiescent for ten centuries there can be little doubt that the steam it evolved was generated by volcanic heat.

The competency of volcanic steam for the production of a crater is thus shown by a parallel instance, and the only conspicuous difference between the Japanese case and the Arizonian lies in the fact that in the one the disrupted rock was volcanic and in the other it was not. This difference seems unessential, for in neither case was there an eruption of liquid rock; the ancient lavas of Kobandai had been cold for ages, and their relation to the catastrophe was wholly passive. Moreover, the manifestation of volcanic energy is no more exceptional on the Arizona plateau than in the Bandai district. The little limestone crater is in the midst of a great volcanic district. (Fig. 1, Page 3, and Plate 1, Fig. 5.) The nearest volcanic crater is but ten miles distant, and within a radius of fifty miles are hundreds of vents from which lava has issued during the later geologic periods.

In following this line of thought I have but reversed the logical route by which Mr. Johnson probably reached his theory, verifying the theory by recomparison with its source.

Yet other verification was afterwards found through the published accounts of certain small craters in Germany, France and India. In the valley of the Rhine are a number of circular basins, for the most part containing lakes and hence called *maars*. They are depressed below the level of the surrounding plain, and some of them are surrounded by raised rims. The descriptions are somewhat conflicting, but it is clear that some of the basins are hollowed chiefly from non-volcanic rocks, limestone, sandstone and slate, and that their rims are composed in part of fragments of similar rocks.* The Indian crater (Plate 1, Fig. 1), which also contains a lake, is hollowed from a volcanic rock, the Deccan trap, and shows no other material; but in other features it parallels so closely the Arizona crater that I quote from Doctor Blanford's description:

"The surrounding country for hundreds of miles consists entirely of Deccan trap; in this rock, at Lonar, there is a nearly circular hollow about 300 to 400 feet deep, and rather more than a mile in diameter, containing at the bottom a shallow lake of salt water without any outlet. * * * * The sides of the hollow to the north and northeast are absolutely level with the surrounding country, whilst in all other directions there is a raised rim, never exceeding 100 feet in height, and frequently only 40 or 50, composed of blocks of basalt, irregularly piled, and precisely similar to the rock exposed on the sides of the hollow. The dip of the surrounding traps is away from the hollow, but very low.

"It is impossible to ascribe this hollow to any other cause than volcanic explosion."†

* Volcanos. By G. Poulett Scrope. London, 1872. Pp. 369-384. Die Vulkane der Eifel, in ihrer Bildungsweise erlautert. By Dr. Herman Vogelsang. Naturkundige verhandelingen von der hollandische Maalschappij der Wetenschappen te Haarlem. Vol. 21, Part 1. Pp. 41–76.

†A Manual of the Geology of India, by H. B. Medlicott and W. T. Blanford. Part I., pp. 379-380 8vo. Calcutta, 1879.

For the sake of completeness, mention should be made of two other hypotheses, which resemble the laccolitic suggestion in that each was based on a single feature of the crater but failed to find verification in any other feature. The fact that the pit occurs in limestone suggested that it might be what is called a *limestone sink*, a cavern having been made by the solution of the rock and the roof having afterwards fallen in.* The fact that the loose debris of the rim lies in hummocks with intervening hollows, and thus resembles in its topographic character the terminal moraine of a glacier, suggested that ice was concerned in its distribution.

Yet another hypothesis, and the last that need be mentioned, was made by welding together two which had preceded. It is a general fact that causes are complex, and as the explanations which first suggest themselves are apt to be simple, it often occurs that the theory finally adopted combines elements of two or more of the theories tentatively proposed. The expert constructor of theories is therefore prone to suspect that rival explanations embody half truths, and to seek for methods of combination. The combination proposed in this case utilizes the theory of meteoric impact and the theory of volcanic explosion, and its author is Mr. Warren Upham. His suggestion is that, by some volcanic process, heat had been engendered among the rocks of the locality, so that the conditions were ripe for an explosion, and that the mine was actually fired by a falling star, whose collision ruptured a barrier between water and hot rock, or in some other way touched the volcanic button.† It will be noted that this explanation demands a coincidence of what may be called the second order, for the colliding star is supposed not only to have chanced upon the prepared

* This suggestion was made by a correspondent.

† American Geologist, Vol. 13, (1894), p. 116; also a personal letter.

locality, but to have arrived opportunely at the critical epoch.

Still another contribution to the subject, while it does not increase the number of hypotheses, is nevertheless important in that it tends to diminish the weight of the magnetic evidence and thus to reopen the question which Mr. Baker and I supposed we had settled. Our fellow-member, Mr. Edwin E. Howell, through whose hands much of the meteoric iron has passed,

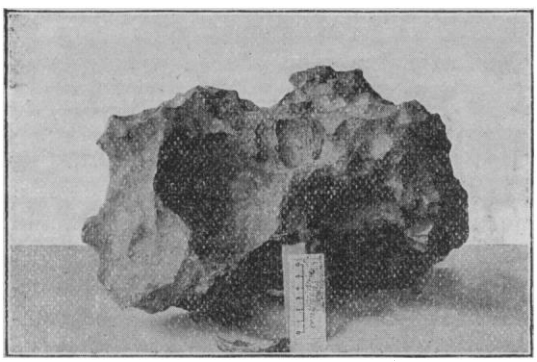

Fig. 5.—Iron meteorite found near the crater. Weighs 161½ pounds. Property of Mr. Edwin E. Howell, of Washington, D. C.

points out that each of the iron masses, great and small, is in itself a complete individual. They have none of the characters that would be found if they had been broken one from another, and yet, as they are all of one type and all reached the earth within a small district, it must be supposed that they were originally connected in some way. Reasoning by analogy from the characters of other meteoric bodies, he infers that the irons were all included in a large mass of some different material, either crystalline rock, such as constitutes the class of meteorites called 'stony,' or else a compound of iron and sulphur, similar to certain nodules discovered inside the iron masses when sawn in two. Neither of these materials is so enduring as the iron, and the fact that they are not now found on the plain does not prove their original absence. Moreover, the plain is strewn in the vicinity of the crater with bits of limonite, a mineral frequently produced by the action of air and water on iron sulphide, and this material is much more abundant than the iron. If it be true that the iron masses were thus imbedded, like plums, in an astral pudding, the hypothetic buried star might have great size and yet only small power to attract the magnetic needle. Mr. Howell also proposes a qualification of the test by volumes, suggesting that some of the rocks beneath the buried star might have been condensed by the shock so as to occupy less space.* These considerations are eminently pertinent to the study of the crater and will find appropriate place in any comprehensive discussion of its origin; but the fact which is peculiarly worthy of note at the present time is their ability to unsettle a conclusion that was beginning to feel itself secure. This illustrates the tentative nature, not only of the hypotheses of Science, but of what Science calls its results. The method of hypotheses, and that method is the method of Science, founds its explanations of Nature wholly on observed facts, and its results are ever subject to the limitations imposed by imperfect observation. However grand, however widely accepted, however useful its conclusion, none is so sure that it can not be called in question by a newly discovered fact. In the domain of the world's knowledge there is no infallibility.

And now let us return for a moment from the illustrative investigation to the hypothesis of hypotheses. If my idea is correct —if it be true that tentative explanations are always founded on accepted explanations of similar phenomena—then fertility of invention implies a wide and varied knowledge of the causes of things, and the

* Mr. Howell's suggestions were communicated orally and are here published by permission.

understanding of Nature in many of her varied aspects is an essential part of the intellectual equipment of the investigator. Moreover, mankind, collectively, through the agency of its men of science and inventors, is an investigator, slowly unravelling the complex of Nature and weaving from the disentangled thread the fabric of civilization. Its material, social and intellectual condition advances with the progress of its knowledge of natural laws and is wholly dependent thereon. As an investigator it makes each new conquest by the aid of possessions earlier acquired, and the breadth of its domain each day is the foundation and measure of its daily progress. Knowledge of Nature is an account at bank, where each dividend is added to the principal and the interest is ever compounded; and hence it is that human progress, founded on natural knowledge, advances with ever increasing speed.

G. K. GILBERT.

SOME FUNDAMENTALS OF NOMENCLATURE.

THE following paragraphs are a brief abstract of two consecutive papers read before the Biological Society, of Washington, on November 16 and 30, 1895. And, though averse to attempting the condensation of so much matter into small space, the attempt is made in deference to the expressed wishes of several who are interested in the questions discussed in the original papers.

It is certainly time that inquiry should be made into the remotest history of the evolution of the binary nomenclature in use by botanists and zoölogists; for it is only by the way of the history of any system that we may easily arrive at an understanding of its fundamental principles. Within the last thirty years there has been much legislation attempted respecting nomenclature. There is talk of further legislation in the future, and certainly much need of it, if by it we may hope to establish a rational and acceptable system. Yet very few of those who enter the arena of nomenclatorial discussion seem disposed to acquire anything more than a superficial knowledge of the origin and development of the binary system; they have never looked carefully to see whether priority, or fitness in names, or the mere convenience of the biological public at a given period, or prevailing usage, is the fundamental principle which has brought the system to its present state; or whether the combined force of all these and some other possible principles have given us such a system—or such a set of systems—as we have, and are more or less content, or discontented.

No subject is well understood, now-a-days, it is everywhere conceded, until it has been viewed from the evolutionary standpoint. But research into the history and evolution of our nomenclature is still neglected; and some are, I think vainly, hoping to resolve all difficulties even by burying still more deeply in oblivion the early history of nomenclature. This is really a curious point in the present status of things. But the present need of historical research is clearly evinced by the absurdities which legislative bodies have already given expression to when endeavoring to state fundamentals.

In attempting to set forth what it calls 'Leading Principles' even the celebrated 'Paris Code' is more remarkable for cheap platitudes and skillful evasions than for any distinct pronouncements regarding principles. Botanists of that period were beginning to awaken to a sense of the importance of priority, but were not yet ready to accord it a place among what were designated as the Leading Principles, yet placing it first among accessory, or secondary, elements of nomenclature.

The body of American botanists who, in 1892, promulgated what is known as the

Rochester Code took a much more decided stand in favor of priority, placing that very word itself foremost in their code. "Priority of publication is to be regarded as the fundamental principle of botanical nomenclature." This language is, nevertheless, not quite so positive as at first reading it might seem. This legislative body apparently wished to say that the principle of priority *is* fundamental, yet did not feel warranted in saying exactly that, but said instead 'is *to be regarded as* fundamental.' Here at once a rather serious question is suggested. Unless priority be quite clearly fundamental, why should a body of scientific men agree *to regard it as* fundamental? In code-making, of whatever sort, everything stands or falls with the ground truth or truths on which the several articles or statutes rest. Error as to the ground principle invalidates every rule and regulation that may be builded on it. Unless some one principle or set of principles may be declared quite positively fundamental, men waste their time in attempting to legislate; the rules are sure to be of little actual force. The authors of the Rochester Code, either consciously or unconsciously, were in a dilemma. They were obliged either to assert that priority is fundamental or else take for the ground principle of their code a mere hypothesis. They chose the hypothesis; and now, until they are ready to erase the hypothetic clause 'is to be regarded as,' each article which depends on the fundamentality of priority is equally hypothetical; that is to say, is no article at all, is utterly without force.

If priority were actually the fundamental principle of nomenclature it would be the chief criterion for the settling of the names of plants and animals; the oldest names would, as a rule, and without respect to other qualities, be maintained. This, however, is far from being the case, even under the working of the so-called Rochester Code. In the case of *Quercus Prinos*, for example, we are employing what is absolutely the latest of the several names that have been given that tree; while the name *Q. castaneæfolia*, which not only enjoys absolute priority, but is also the most appropriate name of all ever given to the tree, is not to be found even in the recent synonymy of the species, and few are aware of its existence; and very numerous instances of this kind could easily be adduced. It may be added, by way of further illustration, that for three centuries the common watercress was known in botanical works by one or the other of the two following names, *Nasturtium aquaticum* or *Sisymbrium aquaticum*. But Linnæus, whom so many people suppose to have been the founder of the binary nomenclature, rejected both these good binary names, disregarded priority, and assigned the species a new and a ternary name, *Sisymbrium Nasturtium aquaticum*. Then again, in 1810, two British botanists sought to reinvest the plant with a binary name; one of these, Sir John Hill, restoring the title *Nasturtium aquaticum*, which had so many centuries of priority in its favor; the other, Robert Brown, giving it still another new designation, *i. e.*, *Nasturtium officinale*, and yet this last, the most recent of all specific names for the cress, is the one which has been sustained everywhere until very recently. Priority certainly is not fundamental when men do again and again in practice so completely ignore it as to seem governed by the very opposite principle, that of taking the newest names instead of the oldest.

The language of the second article of the Rochester Botanical Code is, in several ways, most unfortunate. Its phraseology runs thus: "The botanical nomenclature of both genera and species is to begin with the publication of the first edition of Linnæus' Species Plantarum, in 1753." I do not wish to discuss the absurdity of naming, as initial

for genera, a work in which no genus is defined by description, and in which few or none but the monotypical ones are defined even implicitly by the mention of type species; a book in which the generic names are, therefore, as a rule, *nomina nuda*. It is the unphilosophic handling of certain simple and universal principles, finding expression in logical absurdities, which most impresses the careful reader of the article above quoted. It is manifestly impossible that anything should be made to begin in time that is already past. Whatever affairs are to begin must begin either at once or in the future. Nothing 'is to begin,' or can be made to begin, last year or yesterday any more than in the year 1753. Doubtless the legislators at Rochester would have been glad had they dared to say that botanical nomenclature had its beginning in the year 1753. But they could not have said that. It would not have been true. They might, however, have offered an article which should have read somewhat after this fashion: "It is expedient that, in botanical nomenclature no priorities earlier than the year 1853 be recognized by us henceforward." I have little doubt that this is about what, from their point of view, they must have wished to say. But the situation, thus frankly expressed, would have been too manifestly an embarrassed one. Any number of persons might at once have asked: Why name as an initial date for genera and species a date which is not initial? Or, what expediency can there be in attempting to confine the action of the principle of priority—a principle whose sole force is retroactive—within such narrow limits? It would have been placing priority, previously agreed upon as at least hypothetically fundamental, under great restrictions such as utterly contradict the notion of its fundamentality. Priority is, above all other qualities in a name, the most absolute one, as absolute as the condition of time itself. Its only criteria are dates. If priority be fundamental in nomenclature, then there can be no such thing as an initial date later than the very first beginnings of botanical writing, or publication of names. But, of course, there must be an initial date, a date back of which priorities are to be disregarded; but if this be true, priority is not fundamental, at least not more fundamental than some other principles; very possibly less so. But, having resolved, as our code-makers did, to treat it as being the one ground-principle of the scientific naming of things, they are in a dilemma from the moment of having passed a regulation limiting its action to within what is really a very recent date in the history of nomenclature. The second article of our code, in its real meaning, if it have any, is an almost emphatic contradiction of the first article. It is practically little less than a nullifying of that declaration about the fundamentality of priority, for it excludes, according to credits as given by most learned and eminent botanists of all eras, more than two thousand years of indubitable name priorities, and admits no names as having a history of quite a hundred and fifty years.

The proposition, in itself so perfectly and so evidently true, that priority is determined simply by historic dates—a circumstance which no legislation can alter—brings us back to our initial suggestion, that we can never be prepared to discuss thoroughly the important question of nomenclature, much less be ready to legislate upon this matter rationally and effectually, until we have studied, historically, the evolution of our system of naming plants and animals.

Such historical inquiry would, I think, bring us quickly to the point of acknowledging the principle of convenience—of mere utility—to be the one fundamental thing, which, not only lies at the bottom, but also has chiefly ruled the development

of such systems as we have. This, if found to be the fact, will be very far from yielding the least support to the people who just now, under the name of conservatism, are making the plea of convenience, as against us who would insist upon the exercise of the principle of priority; for they are only pleading as against present changes, that is, against a present and transitory inconvenience, as affecting only the present generation of biologists; whereas, the only convenience which reasonable principles can very seriously regard and try to provide for must be the general convenience of all, that of the future as well as of the present; nay, more than of the present; because it would be absurd to question that the future generations of those who will have to do with the names—scientific names—of plants and animals, are prospectively a thousand fold more numerous and important a body than the whole little handful of to-day, how large a handful we to ourselves may seem.

Of convenience, one of the very prime conditions, as far as relates to nomenclature, is brevity. Such of the Linnæan names of plants and animals as are binary have, by universal consent, been allowed to supersede those older names which were of from three to a dozen words' length; thus has more brevity abundantly proved itself a principle far more truly fundamental than priority.

Again, what is perhaps still more thoroughly an underlying principle of botanico-zoölogical nomenclature is that it be given in the terms of, and according to the rules of, an universal language. It were most easy to demonstrate that neither the binary quality of a name, nor a right of priority, nor both these qualities combined, ever gives a plant name the right to recognition, unless it have the quality of Latinity, unless it be given in the Latin language, at least as to its form. And this, too, is only a matter of general utility; convenience is looked to, not indeed of the English, or of the Germans, or of the Russians, or of the Japanese; for the botanists of each and all these nations, separately considered, would be better accommodated, the English by the adoption of English instead of Latin, the Germans by the adoption of German, as the language of scientific nomenclature, and so on through the whole list of modern tongues.

Under a rational treatment of the whole subject it can hardly fail to appear that, as making for the convenience of the whole botanical world, in time present and to come, the first fundamental principle is that of an Universal Language of Nomenclature; the second, that of Brevity in Names; the third—and this subservient to both the aforenamed, and secondary to them—the principal of Priority of Publication.

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Washington, D. C.

IMPRESSIONS OF THE NAPLES ZOÖLOGICAL STATION.

The *Stazione Zoologica* of Naples is so well known that it is quite unnecessary to say anything at present about the history of this famous establishment. The editor of Science has asked me, however, to write an account of the work of the station as seen from within during my visit of ten months to Naples. During that time it was my good fortune to occupy the table of the Smithsonian Institution, and I take this opportunity to express to the Secretary of the Smithsonian and to the Associated Board of Directors of the Naples Table my indebtedness for the appointment.

Prof. Dohrn has recently given in *Nature* an account of the history of the Naples station and of the work that has been accomplished. Prof. Dohrn's life and interests have been so intimately connected with

the history of the Naples station, and his influence for good is so widely felt throughout the working of the institution that to speak of the success of the station is to speak of the splendid results of the life-work of one man. And all who have been in Naples will, I think, agree with me, when I add that to Professors Eisig and Meyer, no small part of the success of the station is also due.

The station is situated in the beautiful *Villa Nationale*, within a stone's throw of the Bay of Naples. The building of stucco and marble is in two parts joined by a bridge. Within are several well conducted departments. First is the aquarium proper on the ground floor of the larger building. This is open to the public. You enter a large square room with huge aquaria on the sides. In the center are still other and smaller aquaria. Each aquarium is built into the wall, and all the light comes from above, so that the observer standing within the darkened room sees the animals as though himself submerged amongst them. The effect is indescribably beautiful.

The aquaria are supplied with aerated, running water, and it is interesting to note that in winter when the turbid water of the bay is unfit for use, the water in the large reservoir in the station is used over and over again, even for months at a time.

A corps of fishermen is supported by the station and brings in every day fresh material to supply the wants of the investigators and to restock the aquaria. The other Neapolitan fishermen too have learned the value of the rarer animals, and a half-score of these interesting fellows are generally present in the collecting department bargaining, as only a Neapolitan can bargain, for their fish.

All of the Mediterranean forms of life are prepared by the station, and sold at very reasonable rates. Considering the skill required to preserve many of the delicate pelagic animals and the success of Sig. Lo Bianco in this direction, it is not surprising to learn that the Naples station supplies material to many of the largest museums and laboratories of the world.

The chief aim and work of the station is original investigation, and the laboratories are thoroughly equipped for this purpose. There are large zoölogical and physiological laboratories and a smaller botanical laboratory, and in addition a number of private rooms for special research. Each worker is fully equipped with the necessary reagents and apparatus. Peppino is always willing to add any special preservatives, etc., should such be needed. Each worker has a private aquarium for his own use. Every day there is brought in to him a fresh supply of animals.

So rich is the fauna of the bay and so well managed is the collecting department, with its little steamer and other boats, that you have only to make known your wants and you are often embarassed by the quantity of material that is brought to you. Even within the last year the equipment has been overhauled and greatly improved, so that the station is now better prepared than ever before to carry on its work. The number of investigators who go every year to the Naples station is the best guarantee of the widespread appreciation of its advantages. The library is excellent, and the books are made very accessible to all the workers in the station. In the arrangement of the books it is a model of what a library should be. Each investigator is allowed to go to the shelves and get his own books, leaving a card on the shelf in place of the book removed.

The laboratories are open day and night, and the rooms are heated in winter. This is by no means a small matter, for in winter the station is often the only warm place in Naples for weeks together.

The special advantages for work in Na-

ples are I think, these: Absolute freedom to work on any subject desired, a plentiful and never-failing supply of fresh material and a well-filled library always at hand.

At the Naples station are found men of all nationalities. Investigators, professors, privatdocents, assistants and students come from Russia, Germany, Austria, Italy, Holland, England, Belgium, Switzerland and 'America'—men of all shades of thought and all sorts of training. The scene shifts from month to month like the turning of a kaleidoskope. No one can fail to be impressed and to learn much in the clash of thought and criticism that must be present where such divers elements come together. And through all the changes of life and thought Prof. Dohrn and his staff remain always open-minded, courteous, helpful and generous. Isolated, as we are in America, from much of the newer, current feeling, we are able at Naples, as in no other laboratory in the world, to get in touch with the best modern work.

During the ten months in which I was in Naples there were seven Americans there for longer or shorter periods. At present we have but one table under the direction of the Smithsonian Institution. It is needless to add that one table is insufficient for the demands of American students.

The following list gives the names of those who have occupied the Smithsonian Table: Mr. David Fairchild, of the United States Department of Agriculture; Prof. H. C. Bumpus, of Brown University; Prof. Wm. M. Wheeler, University of Chicago; Dr. Lewis Murbach, University of Michigan; Prof. Herbert Osborn, University of Iowa; Prof. T. H. Morgan, Bryn Mawr College; Mr. Walter T. Swingle, United States Department of Agriculture; Dr. J. M. McFarland, Leland Stanford University. The table has been continuously filled since its establishment, and more applications have been made than it was possible to grant.

Williams College at one time subscribed to a table for a year, and the University of Pennsylvania had also a table for a year; and more recently other Americans have enjoyed the advantages of a table subscribed for by Prof. Agassiz.

Major Davis has again and again in recent years most generously paid for tables for those who have been unable to find other opportunity, and it is notorious that for many years in the past the Americans in Naples have had to ask for foreign tables. It is to be hoped that a better time is coming.

T. H. Morgan.

Bryn Mawr College.

ANNUAL MEETING OF THE AMERICAN MATHEMATICAL SOCIETY.

The annual meeting of the American Mathematical Society was held in New York, on Friday afternoon, December 27, at three o'clock, the President, Dr. G. W. Hill, in the chair. Among those present were Prof. Ernest W. Brown, Prof. F. N. Cole, Dr. J. B. Chittenden, Prof. Edwin S. Crawley, Dr. J. W. Davis, Dr. W. S. Dennett, Mr. P. A. Lambert, Mr. G. Legras, Prof. A. Macfarlane, Mr. James Maclay, Mr. C. R. Mann, Dr. Emory McClintock, Prof. James McMahon, Prof. Mansfield Merriman, Prof. Hubert A. Newton, Mr. J. C. Pfister, Miss A. Rayson, Prof. J. K. Rees, Mr. R. A. Roberts, Prof. J. H. Van Amringe, Prof. J. M. Van Vleck, Prof. E. B. Van Vleck, Prof. R. S. Woodward. In the Secretary's report it was stated that the total membership of the Society was 267. The Council and Officers for the coming year are as follows: President, Dr. G. W. Hill; Vice-President, Prof. H. A. Newton; Secretary, Prof. F. N. Cole; Treasurer, Prof. R. S. Woodward; Librarian, Prof. Pomeroy Ladue; Committee of Publication, Prof. Thomas S. Fiske, Prof. Alexander Ziwet, Prof. Frank Morley; other members of the Council, Prof. Henry

B. Fine, Prof. E. Hastings Moore, Prof. Ormond Stone, Prof. Simon Newcomb, Prof. Charlotte Angas Scott, Prof. Henry S. White, Prof. E. W. Hyde, Prof. W. Woolsey Johnson. Prof. B. O. Peirce. The presidential address, delivered by Dr. Hill, was entitled: 'Remarks on the Progress of Celestial Mechanics Since the Middle of the Century.' It will be published in an early number of SCIENCE. Prof. James McMahon read a paper, entitled: 'Note on the separation of the velocity potential (expressed by functions of Laplace and Bessel) into two parts, representing an outward and an inward moving wave.'

THOMAS S. FISKE.

COLUMBIA COLLEGE.

CURRENT NOTES ON ANTHROPOLOGY

RESEARCHES IN SOUTH AMERICAN LANGUAGES.

FROM the rich field of South American linguistics several valuable products have lately been gleaned.

That deserving of the first mention is the narrative of a journey across the Cordillera from Chili eastward, recited in the Huilliche dialect of Araucanian. It was carefully taken down by Dr. Rodolfo Lenz and is printed in the 'Anales de la Universidad de Chile,' Tomo XC. The text, with a literal translation into Spanish, covers 22 pages, and is the first specimen we have, not only in this dialect but in Araucanian, proceeding from the unconstrained lips of a native. It is a model of the manner in which such a piece of work should be accomplished and presented.

The question of the Catamarcan language is again attacked by S. A. Lafone Quevedo in the Anales de la Sociedad Cientifica Argentina, Tom. XXXIX. in an article of 35 pages. He aims to demonstrate from proper names that it is not Kechuan in its affinities. His arguments are drawn from a full investigation of existing fragments of the tongue, and though not conclusive, make an able plea.

A careful vocabulary of the Guaná, from two independent sources, is published by the Reale Academia die Lincei (Rome), this year, the memoir being from the pen of the artist traveler, Guido Boggiani.

A short vocabulary of the Angagueda dialect of the Choco obtained in June last by Mr. H. G. Granger is edited with comparative words by me in the Proceedings of the American Philosophical Society for November.

To these must be added a valuable contribution on the language of the Akua (Chavantes, Cherentes), by Dr. Paul Ehrenreich in the Zeitschrift für Ethnologie, 1895, Heft IV: and several vocabularies from the Orinoco district, published by Dr. A. Ernst, of Caracas, in the American Anthropologist for October, 1895.

THE ANTHROPOLOGY OF WOMAN.

AT the August meeting of the German Society of Anthropology, at Cassel, the opening address was by Dr. Waldeyer, of Berlin, on 'the somatic differences of the two sexes.' Its aim was particularly to bring out the contrasts between woman and man, with the purpose of applying the results to the education and 'sphere' of woman. He argued that since a wide collation of measurements and statistics proves that she has a smaller brain, has less physical strength, preserves more traits of infancy and childhood in adult life, and has practically in all times and places held a position inferior to the man, that in our schemes of social improvement these undeniable facts should be respected. The efforts of social democrats and society leaders to establish entire equality between the two sexes and to throw open to woman all the avenues of activity enjoyed by man, he intimates, are mistaken, and will prove failures; and quotes with approval the opinion

of Bartels, who maintains that the education, physical and mental, of woman, however high it may be, should be always aimed to fit her for the duties of the family circle only.

This conclusion will not be in the least acceptable to the 'advanced' women of the day, nor to those sociologists who see in woman's present condition, not the model of the future, but a survival from a barbaric past.

D. G. Brinton.

SCIENTIFIC NOTES AND NEWS.

EXPERIMENT STATIONS FOR ENGINEERING.

A movement is in progress looking to the development at the 'land-grant colleges' of the several States, of a system of mechanical engineering 'experiment stations,' on much the same basis as the existing agricultural experiment stations organized under the Hatch bill of 1887. It is anticipated that the outcome will be the organization of such stations in all the agricultural and mechanical colleges of the country, in which the agricultural experiment stations have been successfully organized and operated. The purpose of the movement is to secure the promotion of engineering research, and of the development of the scientific facts and principles which are of most value to the mechanic arts and to the profession of engineering. The headquarters of the central office to which all will report is thought likely to be the Bureau of Steam Engineering of the Navy Department; that being the largest, most important and most generally suitable of the government bureaux to take cognizance of such work as is comtemplated. A Department of Mechanic Arts was proposed years ago, probably earlier than the Department of Agriculture, but the importance of the former has not been as promptly or as fully recognized as that of the latter, and nothing has yet been done in that direction. Should such a department be founded, it will naturally become the center of the work of mechanical engineering experiment stations. The present movement has its origin among Southern colleges, and members of the engineering profession who desire to see the encouragement of Southern industries through scientific method, and its earliest expressions is found in the papers of Prof. Aldrich of the West Virginia University, on engineering research.

THE BRITISH MUSEUM.

Natural Science states that the changes at the British Museum (Natural History) on the retirement of the Keeper of Zoölogy, Dr. A. Günther, are as follows: Prof. Sir W. Flower assumes the office of Keeper of Zoölogy in addition to his post as Director, without addition of salary; Dr. Bowdler Sharpe becomes Assistant Keeper of Vertebrata, his department consisting of Messrs. Thomas Boulenger, and Grant; Mr. Edgar A. Smith, Assistant Keeper of Invertebrata, associated with Prof. Jeffrey Bell, Mr. Pocock and Mr. Kirkpatrick; Dr. A. G. Butler, the head of the Entomological Department, with his juniors, Messrs. Waterhouse, Kirby, Gahan Heron, Austen, Hampson, and a new Assistant appointed to fill the vacancy. Mr. Pocock becomes a first-class Assistant. Changes have also been begun in the galleries. The larger fishes will be slung up to the roof, so as not to cumber the valuable floor space, and a more definite arrangement will be made of fishes; similar alterations are contemplated in the reptile gallery, where seventeen crocodiles have for many years enjoyed palatial quarters on the floor. The public gallery of birds will gradually be improved on the plan adopted already is one of the bays, and in the mammalian gallery certain arrangements are contemplated which will show the finer specimens to great advantage. The Trustees have recently purchased for the Department of Geology important series of fossils selected from the collections of the Rev. P. B. Brodie, Rowington, Warwick, and of the late Mr. James W. Davis, Chevinedge, Halifax. Mr. Brodie's collection includes a large number of type specimens described by various authors; and all of these are included in the British Museum selection except those in his unique cabinet of fossil insects, which he still retains. The collection of the late Mr. Davis contains some very fine fishes from the Lower Lias of Lyme Regis and a large number of fragmentary

fish remains from the Yorkshire Coal-measures, described and figured in his own writings.

ASTRONOMICAL.

IN the *Astronomical Journal*, issued December 5th, Dr. Chandler publishes what we may call an ephemeris of the motion of the earth's pole, calculated for the years 1893 to 1896. This ephemeris is arranged in a form admirably adapted for the use of practical astronomers. The simple rectangular coördinates of the instantaneous pole are given for each date, so that it is possible to calculate the instantaneous latitude by means of the very simple formula:

$$\phi - \phi_0 = x \sin \lambda - y \cos \lambda$$

where λ is the longitude.

The numbers in Dr. Chandler's ephemeris are based upon his observational theory of the polar motion. Similar rectangular coördinates of the instantaneous pole, as obtained from actual modern observations, have been computed by Dr. Albrecht, of Potsdam, for the period from 1890.0 to 1895.3. Dr. Albrecht's results were laid before the International Geodetic Commission, which met at Berlin in September. They have not as yet been made generally accessible, though a few copies of his paper were prepared by a lithographic process for distribution among the persons specially interested.

PROF. MAX WOLF recently published in the *Astronomische Nachrichten* an interesting summary of his photographic minor planet work at Heidelberg during the years 1892 to 1895. The observations were made with a six-inch Voigtlaender lens. The total number of plates is 179, with exposures in some cases exceeding three hours. The number of planets found on the plates was as follows:

1892,	38 known planets,	18 new planets.
1893,	27 known planets,	9 new planets.
1894,	15 known planets,	6 new planets.
1895,	19 known planets,	3 new planets.

So it would almost seem that we are approaching the limit of discovery, for planets exceeding the 12th magnitude in brightness.

H. J.

GENERAL.

WE need in America a translation of the recently published work of M. Ch. Letourneau on *La Guerre dans les diverses races humaines.* War is said to have had its origin as a variety of hunting when food, other than human flesh, was unattainable, and when it was comparatively justifiable. M. Letourneau takes for his motto, as a definition of war: *Le vol pour but; le mentre pour moyen*—and he might have added, *la folie pour cause.*

IT is not always that a man during his lifetime learns in how high esteem he is held by those most competent to judge. Dr. Dawson may, therefore, not altogether regret the following editorial article in the *Journal of Geology*: "For the second time in the brief history of the *Journal of Geology*, we are called upon to record the loss of a member of its editorial staff. And now, as before, it is one in the prime of life, in the midst of a brilliant career, and in the enjoyment of rare prospects, Dr. George M. Dawson. Less than a year ago he was elevated to the directorship of the Geological Survey of Canada, a position which he had amply earned by a score or more years of markedly successful work on the Geology of the Dominion. His 'Geology and Resources of the 49th Parallel,' prepared when he was yet a very young man, gave him a recognized place in the scientific world. It has been followed by a long list of papers of unusual merit. It is to Dr. Dawson especially that we are indebted for the geology of the northern Cordilleras and the great northwestern plains beyond the national boundary. His studies lay along many lines, and the wide range of his abilities peculiarly fitted him for the multitude of questions that were presented in the exploration of his vast and varied field. We hope to present a more adequate notice of his work in a succeeding number."

PROF. LLOYD MORGAN, the English biologist, will lecture at Columbia on four Fridays in January, beginning January 10th. His subjects will be: 1. 'Illustrations of Instinct.' 2. 'Some Habits and Instincts of Young Birds.' 3. 'The Emotions in their Relation to Habit and Instinct.' 4. 'Some Instinctive Activities of the Pairing Season.' The lectures include a discussion of his own experiments and opinions upon the Darwin-Spencer theory of 'instincts as inherited habit.' His lectures before the

Lowell Institute in Boston will be delivered upon Tuesdays and Saturdays in January, beginning January 7th. He will also lecture at Brown University and at the University of Illinois. Letters addressed to the care of Columbia College or of the Lowell Institute will reach him.

THE one hundred and twenty-eighth Bulletin of the United States Geological Survey is a review of the Bear River formation and its characteristic fauna by Charles A. White. The author states that his object is the correction of an essential error which has long prevailed among geologists concerning the taxonomic position of one of the North American Cretaceous formations; that is, its object is to present a summary of the facts which show the entire separateness from the Laramie formation of that series of non-marine strata which has heretofore been known as the Bear River Laramie, with which formation the Bear River series of strata has long been confounded. To this end the Bear River series is defined as a distinct formation, stratigraphically, geographically and paleontologically, and its taxonomic position is stated in detail.

M. GEORGES LEMOINE reported to the Paris Academy on December 2 that he had measured the amount of decomposition caused by light in solutions of ferric chloride and oxalic acid, and had found the rate of decomposition to be approximately proportional to the intensity of the light. We are not informed how the intensity of the light was measured, but if the chemical action of light can be used to measure luminosity it would be an important photometric method. The photochemical and luminous intensity of light do not, however, remain proportional when the wave-length is altered.

IN a recent work on Meteorology in its relation to Hygiene, Dr. Van Bebbier states that the average total number of hours of sunshine per year is in England 1,400, in Germany 1,700, In Italy 2,300 and in Spain 3,000. In a hundred possible hours of sunshine there are in London on the average 23 and in Madrid 66.

AT a recent meeting of the Paris Academy MM. Troost and Ouvrard reported that they could only discover faint or doubtful traces of the spectrum lines of helium in sea water or in water from the Seine. It seems to follow that the helium in the mineral springs of Canterets cannot be attributed to the air, but comes from the rocks with which the water has been in contact.

DR. FAUVEL, born at Amiens in 1830, a specialist on diseases of the throat and nose and the author of important works on these subjects, died in Paris on December 17. On the same day the death occurred of Dr. Vandermey, professor of gynecology in the University of Amsterdam.

THE *British Medical Journal* states that Dr. A. J. Woïtoff, professor of bacteriology in the University of Moscow, recently fell a victim to his devotion to scientific research. He infected himself with a virulent culture while experimenting in his laboratory, and died soon afterwards of the effects of the accident.

THE life of Darwin, written by Prof. Wilhelm Preyer, has been published by Ernst Hofmann, Berlin.

'THE Earth's History,' by R. D. Roberts, of Cambridge University, and 'The Realm of Nature,' by Hugh B. Hill, are announced for publication by Charles Scribner's Sons.

IT is stated that the New York Pasteur Institute has purchased a farm of about 200 acres near Tuxedo Park to be used as an experiment station.

DR. D. MORRIS, Assistant Director of the Kew Gardens, delivered a lecture on 'The Rise and Progress of the Royal Botanical Garden at Kew, England,' at the American Museum of Natural History under the auspices of the New York Botanical Garden, on December 17th. Dr. Morris has now gone to the Bahama Islands, in order to investigate the cultivation of hemp and other products of the islands.

THE *British Medical Journal* summarizes in the issue of December 14th statistics which have been collected by Widmark regarding blindness in Scandanavia. These show that Denmark had in 1890 for every 10,000 inhabitants only 5.3 blind, Sweden 8.3, Norway 12.8, Finland 15.5. Compared to other European countries, of which Portugal and Russia stand highest with

20 blind for every 10,000, and Holland lowest with only 4.5, the order is as follows: Portugal, Russia, Finland, Spain, Norway, Hungary, England, Germany (without Prussia), France, Prussia, Sweden, Belgium, Austria, Switzerland, Italy, Denmark and Holland.

A SWISS National Exposition, promoted by the Swiss Confederation and the different cantons and cities, will be held at Geneva from May 1st to October 15th of the present year.

AT the 252d regular meeting, held Saturday, December 28th, the Biological Society of Washington elected the following officers for 1896: President, Surgeon General Geo. M. Sternberg; Vice-Presidents, Richard Rathburn, C. D. Walcott, L. O. Howard, B. E. Fernow; Recording Secretary, M. B. Waite; Corresponding Secretary, F. A. Lucas; Treasurer, F. H. Knowlton; Members of the Council, F. W. True, C. W. Stiles, W. H. Ashmead, F. V. Coville, C. L. Pollard.

THE New York *Evening Post* states that one of the greatest of the world's bridges is to be built at Detroit, to connect that city with Windsor. It is to be over two miles in length and to be five feet higher than the Brooklyn bridge. The plans for the structure have been prepared, and legislation looking to its construction has been asked in Washington and Ottawa. A corporation has been or will be organized under Michigan law to coöperate with a similar Canadian corporation in constructing the bridge, and the Vanderbilts will guarantee the bonds of both. The estimated cost is between four and six millions.

THE *Journal of Geology* announces that it will publish, beginning with the first number of Vol. IV., a series of four articles under the head of 'Studies for Students,' by Prof. Van Hise, on (1) Movements of Rocks under Deformation; (2) Analysis of Folds; (3) Cleavage and Fissility; (4) Joints and Faults.

THE *American Machinist* states that a bill has been introduced in the United States Senate by Senator Quay asking for an appropriation of $25,000 for the Franklin Institute and Purdue University, for the purpose of determining the quantity and effect of hammer blow, 'centrifugal lift and tangental throw' of locomotive wheels in use on American railroads; also the effects produced thereby.

THE *Appalachian Mountain Club* announces that it will publish in the early spring a 'Guide to Walks in the Country about Boston,' covering practically the ground embraced in the Club map of the country about Boston. The book will have many maps and be illustrated, and it is desired to have as many of these illustrations as possible taken by the amateur photographers of the Club.

UNIVERSITY AND EDUCATIONAL NEWS.

THE *Evening Post* states that at a meeting of the committee on buildings of the American University, architects have been chosen to prepare plans for the hall of the history building. A subcommittee was also chosen to take charge of the construction of the structure, which will cost about $150,000. Bishop Hurst announced an additional gift to the University, that of a business block in Findlay, Ohio, valued at $10,000, from John D. Flint, of Fall River, Mass.

MRS. T. K. W. SHIMER, owner and principal of the Mount Carroll Female Seminary of Mount Carroll, Ill., has offered to the University of Chicago the seminary buildings and twenty-five acres of ground, with an endowment of from $150,000 to $200,000, to be a girls' training school in connection with the University.

MR. SIDNEY A. REEVE, for several years employed with the engineering firm of Westinghouse, Church, Kerr & Co., and recently editorial writer on the *Progressive Age*, a journal devoted to gas interests, has been elected adjunct professor of steam and hydraulic engineering in the Worcester Polytechnic Institute. Prof. Reeve will begin his services in the Institute about January 1st, 1896.

MR. LECKY, the historian, has been elected member of Parliament for the University of Dublin by a majority of 750 votes.

DR. N. KUSNETZOFF has been elected associate professor of botany and director of the botanical gardens in the University of Dorpat.

A NEW educational review has appeared at Leipzig, *Deutsche Zeitschrift für Ausländisches Unterrichtswesen*, edited by Dr. J. Wychgram.

SCIENTIFIC LITERATURE.

Justus von Liebig, His Life and Work (1803–1873). By W. A. SHENSTONE, F. I. C., Lecturer on Chemistry in Clifton College. New York, Macmillan & Co. 1895. Pp. 220 + vi.

This is one of 'The Century Science Series' edited by Sir Henry Roscoe, and it is fitting that one of the first chemists to receive attention should be Liebig. In his preface the author says: "The name of Liebig is doubtless familiar to most of us, but I fear very few have any clear idea what he did, why chemists admire and esteem him, or, indeed, are aware that they do admire and esteem him. As the result of many inquiries made among cultivated people, I have found the prevailing impression concerning Liebig to be that he was a man who gained a large fortune by making 'extract of meat.' Now and then one meets someone who 'seems to have heard' of his name in connection with agriculture. Scarcely anyone now seems to know that he was one of the greatest of that class in whose work Mr. Balfour finds 'the causes which more than any others conduce to the movements of great civilized societies.' I have therefore made it my object in writing this little book not so much to dwell upon Liebig's private life as to tell what he was, what he did, and why all chemists and all those who are versed in the history of science admire and esteem him so greatly."

There can scarcely be a doubt that chemistry owes more to Liebig for its advancement during the present century than to any other one man. He was born in 1803 at Darmstadt, where his father dealt in colors, which he also manufactured. The boy was a failure at school. He had no ear memory and could not, therefore, make progress in linguistic studies. On the other hand, he had the powers of an experimenter, and was attracted by everything connected with chemical phenomena. He spent some time in an apothecary shop, but he took little interest in the commercial side of his occupation, and, in the course of a few months, he was sent back to his father. It was then decided that he should follow his bent and study chemistry. He went to the Universities of Bonn and Erlangen, but did not find what he wanted. In 1822 he took the degree of Doctor of Philosophy at Erlangen, and then he was provided with the means for continuing his studies abroad. He went to Paris and was soon admitted to the laboratory of Gay-Lussac, one of the leading chemists of that time. Two years later he was appointed Extraordinary Professor of Chemistry at Giessen. In 1826 he became full professor. In 1852 he was called to Munich, where he died April 18, 1873.

"Liebig was essentially a pioneer in science. In the course of his life he took the lead in no less than four great departures. The first was in organic chemistry, the second and third in the applications of chemistry to agriculture and to physiology, the fourth was the outcome of his labors as a teacher."

How he labored in these four fields is well told in Mr. Shenstone's little book, and every one interested in the intellectual development of mankind, be he chemist or not, will find here much that is stimulating and suggestive. The book is divided into nine chapters with the following titles: Introduction; Liebig and Wöhler; Chemical Discoveries; Liebig and Dumas; Fermentation; Chemistry and Agriculture; Physiological Chemistry; Education and Other Work; Character and Later Years.

Anleitung zur mikrochemischen Analyse der wichtigsten organischen Verbindungen. VON H. BEHRENS. Prof. an der Polytechnischen Schule in Delft. Erstes Heft (Anthracengruppe, Phenole, Chinone, Ketone, Aldehyde) Mit 49 Figuren im Text. Hamburg und Leipzig. Verlag von Leopold Voss. 1895. Pp. 64+viii.

The author of this book is well known in connection with work on microchemical analysis in general. He has now endeavored to show the chemist who deals with organic compounds how he may avail himself of the microscope for the purpose of recognizing various substances. The methods described have been thoroughly tested in the author's laboratory and the results have been most satisfactory.

The refinement attainable is not equal to that reached in the case of inorganic compounds. One cannot think of working with millionths of milligrams, and will at times have to be content if a satisfactory result is reached with tenths of milligrams. The classes of compounds dealt with, in this first number of the book, are: 1. The anthracene group; 2. Phenols; 3. Nitro-compounds; 4. Quinones, Ketones, Aldehydes. It is to be hoped that the appearance of the book will lead chemists to try the new methods, as it appears that their work will be much facilitated by them. It must, of course, be borne in mind that the problem of detecting minute quantities of organic compounds does not often present itself, though there are cases in which it becomes of importance. IRA REMSEN.

On the Densities of Oxygen and Hydrogen and on the Ratio of their Atomic Weights. By EDWARD W. MORLEY, Ph. D. Published by the Smithsonian Institution, Washington, D. C. 1895. 4°. xi. 117 pp.

For more than ten years Prof. Morley has been almost constantly engaged on the work which is described in this paper. With a painstaking fidelity to the highest ideals of accurate work which has rarely been equalled and has never been surpassed, he has determined four constants which are partly interdependent, and which are of very great importance in physical science. These constants are: the density of hydrogen, the density of oxygen, the ratio of the combining volumes and the ratio of the combining weights of the two elements.

The density of oxygen was determined by three different methods.

In the first series nine determinations were made. From nine to twenty-one and one-half liters of oxygen were weighed in large globes which were filled at the temperature of the laboratory.

In the second series sixteen determinations were made. Instead of measuring the temperature and pressure directly in this series the oxygen was brought to the same temperature and pressure as that of hydrogen contained in another large globe. The pressure of the hydrogen was previously measured at the temperature of melting ice, thus making the globe containing it, in effect, a very sensitive air thermometer. The difference between the coefficients of expansion of hydrogen and of oxygen was of course considered.

In the third series seventeen determinations were made. The globes were filled at the temperature of melting ice and, after weighing them filled with oxygen, they were exhausted and weighed again. The oxygen in this series was prepared partly from potassium chlorate and partly by the electrolysis of dilute sulphuric acid.

The results of three series were:

By use of thermometer and manometer	$D = 1.42879$
By compensation	$D = 1.42887$
By use of ice and barometer	$D = 1.42917$

Giving double weight to the last series, the weight of a liter of oxygen under normal conditions at sea level and in latitude 45° is 1.42900 grm., with a probable error of 0.000034 grm.

Five series of determinations of the density of hydrogen were made.

In the first and second series the same methods were used as in the first and third series for oxygen.

In the third, fourth and fifth series hydrogen was absorbed in palladium, contained in a glass tube, and, after weighing, was expelled into three globes which were surrounded with melting ice, and which had a combined capacity of forty-two liters. By this means three and seven-tenths grams of hydrogen were weighed in a comparatively small apparatus, and the volume occupied by the gas was accurately determined. The method has the additional advantage that any mercurial vapor contained in the globes was without effect on the determination. In all, sixty-four determinations were made. The results were as follows:

Series	I.	$D = 0.089938$
"	II.	$D = 0.089970$
"	III.	$D = 0.089886 \pm 0.0000049$
"	IV.	$D = 0.089880 \pm 0.0000088$
"	V.	$D = 0.089866 \pm 0.0000034$

It is believed that mercurial vapor entered the globes in the first two series and that the results of those series are too high. They are accordingly rejected. The remaining series give as the weight of a liter of hydrogen at sea

level in latitude 45° and under normal conditions, 0.089873 ± 0.0000027.

In 1891 Prof. Morley published* a series of determinations of the volumetric composition of water. The results of these determinations were extremely concordant and there can be no reasonable doubt that the same ratio would be obtained again by the same method. When, however, this ratio is combined with the ratio of the densities given above, the resulting value for the atomic weight of oxygen does not agree with that which Prof. Morley has obtained by the direct weighing of oxygen and hydrogen and of the water formed by their union. Scott has recently determined† the volumetric ratio and finds the value 2.00285. This ratio, when combined with the ratio of densities as found either by Lord Rayleigh or by Prof. Morley, gives the same value for the atomic weight as that found by the gravimetric method. Prof. Morley has, therefore, determined the volumetric ratio by another method. In a series of ten experiments he determined the density of electrolytic gas obtained from a solution of caustic potash. He also determined the excess of hydrogen present in the gas. From the results obtained, and, taking into account the change in pressure occasioned when one volume of oxygen is mixed with two volumes of hydrogen and the mixture is made to occupy three volumes, the value 2.00269 for the volumetric ratio was calculated.

It seems to be established, therefore, that the values obtained by Prof. Morley with the eudiometer were not correct as representing the volumetric ratio and that the density of a gas in a tube is different from that in a globe, the effect on the density being different for a light gas from that for a heavy one.

The gravimetric composition of water was determined in a series of twelve experiments. In these the oxygen was weighed in large globes, the hydrogen (three and one-half grams), in palladium, and the two gases were burned in an apparatus so devised that the water formed was also weighed. In this way each experiment gave two independent determinations of the atomic weight of oxygen.

* Amer. Journ. of Science, *41*, 220.

† Phil. Trans. *184*, A, 543 (1893).

The results were:

From the ratio of hydrogen and oxygen,	15.8792
From the ratio of hydrogen and water,	15.8785

These values agree to the third decimal with the value calculated from the volumetric composition and the ratio of densities as given above.

The final results of Prof. Morley's determinations are:

	Grams.
Weight of one liter of oxygen, latitude 45°,	1.42900
Weight of one liter of hydrogen, latitude 45°,	0.089873
Atomic weight of oxygen, chemical method,	15.879
Atomic weight of oxygen, physical methods,	15.879
Molecular weight of water, chemical method,	17.879

In conclusion a summary of previous determinations of the constants in question is given. Omitting the earlier determinations, which were manifestly inaccurate, and the results of one more recent experimenter, whose work appears to have been affected by some source of constant error, the mean of all the other determinations of six different observers gives the value 15.879 for the atomic weight of oxygen.

It is impossible, in a brief sketch of this kind, to convey any adequate idea of the pains which was taken at every step to secure the greatest possible accuracy in the work, nor of the genius which has been displayed in devising complicated apparatus adapted for the determinations to be made. The work is classical and must, hereafter, be consulted by every one who wishes to do the best work in this field.

W. A. Noyes.

EIMER'S EVOLUTION OF BUTTERFLIES.*

Prof. Eimer, of Tübingen, is an enthusiastic opponent of Darwin's theory of Natural Selection, and has a theory of his own to replace it. The theory of Eimer has been defended by him on various occasions, his main exposition being given in his work on the origin of species published in 1888. His investigations on butterflies (thus far of the genus Papilio *auct.* only) are intended to afford proof of his theory in a

* Die Artbildung und Verwandtschaft bei den Schmetterlingen. II. Theil. von Dr. G. H. Theodor Eimer unter Mitwirkung von K. Fickert. Text 8vo. Pp. viii, 153. Atlas Folio Tafeln v.–viii. Jena, Gustav Fischer. 1895.

special case. His standpoint is indicated in his preface, in which he says:

"My butterflies demonstrate, as said above, the impotence of natural selection over a wide territory; their formation of species occurs evidently without any influence of Darwinian selection, and, therefore, disproves Darwinism completely. * * * * *There is no origin of species by natural selection*, but only a preservation of species already existing. The assumption that natural selection can bring forth new species rests upon a gross defect of reasoning (Denkfehler). Natural selection cannot cause new species to arise, either by the formation of new characteristics or by the division of existing chains of organisms into species. My butterflies show, in complete contradiction to the Darwinian doctrine, that new characters arise by development in a few predetermined directions (*Orthogenesis*) or by organic growth (*Organophysis*) from physiological causes. They show that it is essentially a still stand (*Genepistase*) at determinate stages of development, which separates a chain of organisms into species, together with certain other causes, such as the preventing of impregnation (*Kyesamechanie*) and development by jumps (*Halamatogenesis*)." *

He also claims that he presents only facts—no suppositions or hypotheses:

If Prof. Eimer's claims are correct, his researches mark one of the great epochs of biological discovery. It is, therefore, desirable to determine with precision the nature and value of the evidence which he presents.

The study of his work on butterflies (including both the present second part and also the first, published in 1889) shows that the facts of actual observation are solely the markings and geographical distribution of species of the genus Papilio *auctorum*. From these observations our author has deduced a systematic arrangement of several groups of species, so as to present them in what he believes to be their true phylogenetic relationships. From the standpoint of the systematic entomologist Eimer's work is certainly both interesting and valuable, since the figures and descriptions are very painstaking, and his groups are natural ones, and we may even go further and admit that his grouping of the species is in the main correct. Herewith we come, not without some surprise after the assurances of the preface, to the end of Eimer's positive facts. The remainder of his book is constructed of interpretations of the facts, and these interpretations cannot be designated otherwise than as a series of unproven assumptions and hypotheses. We may indicate the reasons for this characterization by a few illustrations of his reasoning. Thus he states (pt. i, p. 2) that in all animals longitudinal stripes are primary markings, longitudinal rows of spots secondary, and transverse markings tertiary. By this rule he is able to decide easily which living species of Papilio are nearest the ancestral forms. Surely such a universal rule needs to be demonstrated, not proclaimed *ex-cathedra*. His laws of the genesis of species are deduced thus: In a series of species of Papilio there may be ancestral forms with much black and descendent forms with little (Antiphates group), or just the other way the descendants blacker than the ancestors (Leosthenes-Ajax group) yet all the species concerned are living and no *proof* is offered that this or that form is ancestral, we are simply told that it is so. Again he finds a series of species, which differ from one another by the width of certain dark bands, each species taking its place according to the width of the bands. Such a series is his proof of *halmatogenesis*, and he entirely passes by the possibility that there may have been intermediate forms with the simple denial of their existence. Now it is certainly possible that the species of Papilio arise by discontinuous variation, to use Bateson's felicitious term, but between what seems possible in the present state of our knowledge, and absolute certainty there is a vast abyss, across which Prof. Eimer airily makes his way with the bare affirmation 'my butterflies prove halmatogenesis.' Not a word throws any light on the question how do they prove it?

Prof. Eimer lays stress upon the direction of the assumed development of a series of forms, and from the fact that a series of species may exhibit progressive increase in a certain character, he infers that the progress is a predetermined development. He overlooks this simple consideration *that no matter how evolution is caused it must be in some direction, and the mere observation of that direction cannot prove that there was a predetermining tendency to the ances-*

* Slightly abbreviated. The italics are the author's.

tral form to develop in that direction. Again a difficulty is encountered when we examine another of our author's fundamental principles, the inheritance of acquired characteristics, because the *assertion* of this principle is made and yet no demonstration of its truth is offered—it is at best a bold hypothesis.

Another peculiarity of the author's position is his serious misapprehension of Darwin's theory, which he mistakes repeatedly. He rejects Darwinism because it does not explain the origin of variations. Darwin, of course, did not attempt to more than suggest certain explanations, and his theory of natural selection does not depend on the origin of variations, but on the demonstrated fact that innumerable variations do occur and numerous variations have been transmitted. Prof. Eimer claims that his book should be 'read *and studied*,' in return we claim that before he again writes against Darwin, he should thoroughly master Darwin's chief work, the '*Variation of Animals and Plants.*' Until he has done that his attacks must remain unheeded, for they are only against a straw substitute for Darwinism.

Professor Eimer's book is a valuable contribution to descriptive entomology, and sets before entomologists a high standard of description and illustration of species. It is also an unsuccessful attempt to substitute for Darwinism a new theory of evolution, based wholly upon hypothetical assumptions, for no one of which is substantial proof offered, and so far from agreeing to the author's claim that his theory is a series of facts, we must, on the contrary, say that it is a collection of arbitrary assertions. He condemns Weismann very emphatically for speculating, and yet shows himself, perhaps, the more speculative of the two.

C. S. Minot.

The Structure and Life of Birds. By F. W. Headley, M. A., F. Z. S. London, Macmillan & Co. 1895. 8vo. Pp. xx. 412. 78 illustrations. $2.00.

This book "attempts to give good evidence of the development of birds from reptilian ancestors, to show that modifications in their anatomy have accompanied their advance to a more vigorous life, and, after explaining, as far as possible their physiology, to make clear the main principles of their noble accomplishment, flight, the visible proof and expression of their high vitality. After this it deals, principally, with the subjects of color and song, instinct and reason, migration and the principles of classification, and, lastly, gives some hints as to the best methods of studying birds."

Mr. Headley's aim is confessedly an ambitious one, and since he has shot so well he must not take it amiss if he is told that his pen has not carried quite true throughout its entire flight. It is difficult to compress so many subjects as are contained in the 'Structure and Life of Birds' into the compass of four hundred pages, and we can not expect to have every point touched on fully and clearly explained. Still, making due allowance for this and for the popular audience to which the book is largely addressed, there is a certain amount of looseness, or inexactness, of statement that might have been avoided. For example, uncinate processes are not 'common to all birds,' since they are absent in the Screamers, a fact which might have been explained in half a dozen words. Neither is the supplementary toe of the Dorking Fowl a dermal bone, but a case of duplication of a digit, the perpetuation by careful breeding of an abnormality now and then seen among animals, even in man. This looseness of diction is well shown by the constant reference to bones filled with marrow as *solid* bones; and the statement that the coracoid and clavicles are firmly fixed to the breast bone, when this is rarely the case; and those birds in which the clavicle is most securely fastened to the sternum are by no means among our best birds of flight. The statement that all the bones of the Swallow are filled with marrow is a little indefinite, and if intended as generalization, misleading, since the humerus may be pneumatic, even among Swallows.

However, pneumaticity is a very inconstant character and is not even of generic value. The connection between the reduced phalanges of the Swift and its alleged inability to rise from the ground is not clear to the average mind, and it is rather startling to be told that the Rook may be told from the Crow by the absence of feathers on the beak.

It would also have been well to have avoided positive statements concerning facts or theories still in debate, especially such an one as that the skull is no doubt partially made up of vertebræ, or that the pisiform is an ossified tendon. Neither do we *know* that an insect gets a mosaic picture of an object, while, had Mr. Headley heard the question of the sense of smell in *Cathartes* discussed, he might not be so certain that vultures do not scent carrion from afar, although neither that nor the contrary is yet proven. In discussing flight too much stress is laid on the importance of the clavicle. As the author states, the bone is rudimentary in parrots which fly exceedingly well, while any one who has dissected humming-birds will be morally sure, from its shape and insignificant proportions, that these birds could dispense with the bone. We are told that the wing serves as a parachute to sustain the bird between the strokes of the wing and, but for this, the drop would be greater than it is. A more obvious explanation would seem that there is not sufficient time for gravity to overcome the inertia of onward movement, for it is very evident that unless a bird is falling more rapidly than the wing is being raised, the wing can afford no support. Many other things might be said—did space allow—concerning the chapter on flight, but it will suffice to remark that there is as yet no proof that the muscles of birds exert any unusual power; on the contrary, birds which like the larger petrels have mastered the problem of sailing flight, not only have small wing muscles, but have very little strength in them, and it was pleasing to obtain from Prof. Moseley's notes corroborative evidence of the inability of the Cape Pigeon (*Daption*) to rise from the water after a hearty meal.

A word or two on another point. Why does Mr. Headley confuse the reader by calling both the leg of a man and the wing of a bird the homologue of the arm, when a better and clearer expression would be that the fore limb is the homotype of the hind?

But in spite of blemishes, some of which have been cited to warn the reader to be on his guard, and to use a pinch of salt now and then, the 'Structure and Life of Birds' is a most interesting book and a welcome addition to ornithological literature. Many of the errors may be ascribed to the fact that the author is so brimful of his subject that, writing as he does *calamo currente*, his ideas outstrip his pen and are incorrectly recorded. The style is bright, clear and readable, the illustrations illustrate and are not thrown in, while the numerous bibliographical references are not only a boon to the reader who would like to know how he may best extend his knowledge, but to him who would like to know on whose authority some of the statements are made. The book is evidently based on much observation and experiment, supplemented by a vast amount of reading, and it will give the general reader, and many a one who considers himself an ornithologist, a good idea of many of the facts and problems concerning birds. The reader will learn why the perching bird does not fall from the bough, even when asleep, will find full details of the wonderful air sacs with which the body is permeated, and much information as to how a bird breathes and how his blood circulates. He will gather that the colors of feathers are due to a variety of causes, and learn that they correspond to the scales of snakes as well as much of their growth and mode of shedding and renewal.

The chapter on flight is particularly full and interesting and this difficult matter is well treated, and it is to be hoped that the concluding chapter may stimulate some, at least, of its readers to address themselves to some of the many branches of ornithology which lie ready to their hand. Lastly, but by no means least, the book is well indexed.

F. A. LUCAS.

The Beginnings of Writing. By WALTER JAMES HOFFMAN, M. D. With an introduction by PROF. FREDERICK STARR. New York, D. Appleton & Co. 1895. Pp. xiv+209.

In this latest volume of the 'Anthropological Series' Dr. Hoffman has attempted to present in brief and popular form the results up to date of the researches into the origin of the art of writing.

The development of the use of conventional signs is traced from pictographs through symbols, mnemonic signs, etc., to alphabets, and

the result is a work not only of interest to the lay reader, but of considerable scientific merit and usefulness. The difficulty of selection from the mass of material, much of it of doubtful interpretation, to say the least, which the author had at his disposal must not be underestimated and to say, as we may, that he has accomplished his task with judgment is no mean praise.

The first four chapters of the book are taken up with a discussion of pictography, both descriptive and interpretative, and here, as was to be expected from the previous work of the author in the picture-writing of the North American Indians, he shows himself thoroughly at home. One of the main faults of the book may be mentioned here, and that is the almost overwhelming prominence given to American remains and records in nearly every question under discussion, a fault easy to understand when the volume of research and even relative importance of the pictographic remains of the aborigines of this and other countries is considered, and yet the idea of proportion which the general reader would obtain from the book must inevitably be a wrong one.

Of pictographs on stone those of the 'Algonkian type' are the most numerous and widely distributed, corresponding to the great area occupied by tribes of the linguistic family of that name. They appear to be mainly representations of animals or concrete objects and probably served as hunting or other records. The author points out that in nearly every instance these Algonkian petroglyphs have been placed upon rocks low down along the shore of water courses, whereas many of the pictographs of other types are placed upon high and conspicuous cliffs, in which case the drawings are apt to be colored.

In Mexico and Central America, petroglyphs are comparatively rare, while in South America investigation is at present not far enough advanced to present examples of much importance.

In the chapter on pictographs on materials other than stone, the art is traced through carvings and drawings on ivory, bone and shell, in which the Alaska Innuits especially excel, through birch bark records to the use of maguey paper by the Mexicans and papyrus by the Egyptians. The Mexican pictographs show a very high degree of development in which the artists had passed the stage of mere concrete object drawing, and show signs of a beginning system midway between the pictographic and the phonetic. This system which has been called the 'ikonomatic' is one in which "the object employed to represent a complex word or character, each furnishes its first syllable, or more, to suggest the sound required for the complex character and may have no other relation to the general result." Colors were largely used and may have had a phonetic value, though often were nothing more than the natural color of the object depicted.

Dr. Hoffman denies that any evident parallel exists between the pictographs of the Western hemisphere and those of the East. The Egyptian had become entirely phonetic and partly alphabetic, while the Chinese and other systems were of a well developed syllabic order; the American aborigines, on the other hand, had not yet risen above the stage when a study of the origin of their pictographs is possible, and therein lie their peculiar interest and value.

The chapters on symbolic signs and gesture signs and attitudes are especially good and well arranged, while those on the growth of conventional signs and comparisons give interesting examples of primitive designations from which space prevents our quoting.

The book closes with a discussion of the growth of the alphabet through the various stages of graphic development; the transition stages where the alphabetic character has served as a pictorial representation of an object and as a syllable being proved, as indicated above, by reference to the systems in use among the early Mexicans and the Mayas of Yucatan.

Ikonomatic or rebus writing was extensively used by the Mexicans, while the Mayas went a step further and employed purely phonetic signs as well as ideographic characters.

In conclusion it may be said that Dr. Hoffman has raised very markedly the standard of the hitherto somewhat disappointing series in which his book appears, a standard which it is to be hoped the succeeding issues will sustain.

Livingston Farrand.

Columbia College.

SCIENTIFIC JOURNALS.

JOURNAL OF GEOLOGY, NOVEMBER–DECEMBER.

The Greenland Expedition of 1895: By R. D. SALISBURY. This is not an itinerary of the expedition, but a discussion of the geologic and geographic problems suggested in the course of it. The following are considered: coastal topography and its interpretation, evidence concerning past glaciation from nature of rock surfaces, general distribution of snow and ice, icebergs and evidences concerning recent changes of level. The time allowed was not sufficient for detailed observation on any of these lines, but the facts gathered are of especial significance as supplementing and checking as well the work of last summer in this little known field. The author finds strong evidence that the Pleistocene ice sheet of America did not come from Greenland, and that the conditions for glaciation on the coast of Greenland to-day are better in latitude 74°–76° than in 76°–79°. Another interesting conclusion is that the ice cap of Greenland did not reach its greatest extension at all points at the same time. The observations on icebergs are quite full and show clearly either that there was little debris in the parent glaciers, or that it was quickly lost by the bergs.

A Circum-Insular Paleozoic Fauna: By S. WELLER. So long as paleontology made the identification of species an end in itself and assumed that forms found widely separated in space must belong to different species, even though they seemed to be identical, it was necessary doubtless, but it was not interesting to the philosophic geologist, because it seemed to him to ignore more than it considered. In later years there has been a decided broadening in the view of paleontologists. Under the lead of Williams, Walcott and Smith in this country there has been an attempt to solve the same kind of problems for ancient faunas and floras which Wallace, Darwin, Gray and others have solved for modern ones. In this paper the author applies the method in determining the origin of the Chouteau fauna of the Ozark area in southeastern Missouri. He finds evidence of a land barrier extending from 'Isle Wisconsin' southwest through this area in early Devonian time which separated two rather distinct faunas. In the latter part of the Devonian this land barrier became sea bottom, and the two faunas mingled freely in the Ozark area. The result was a new fauna decidedly Carboniferous in its affinities, though Devonian in time. The most hardy elements of the two competing faunas survived, and this new vigorous stock gave character to succeeding faunas for a long period. Some pregnant suggestions are made regarding correlation of formations.

Experiments in Ice Motion: By E. C. CASE. The mechanics of glacier motion involve questions often asked but not easy to answer. The experiments of the author were designed to throw light on the existence and nature of differential movement in the basal portions of glaciers. Paraffine with a quantity of refined petroleum to lower the melting point was the material used. It was placed in a box with various obstructions in the bottom and by means of a close fitting plunger was forced toward the middle of the box over the obstructions. In order to trace the currents, thin lines of powdered coal or Galena, and layers of dark wax, were used. The results, as shown by the photographs, tally well with Prof. Chamberlin's descriptions of some Greenland glaciers. The author finds proof of both vertical and horizontal differential movements in the basal portion of the wax. Similar currents in glaciers he thinks may be the cause of certain features of subglacial topography. For example, he finds that drumlin areas lie in the lee of escarpments or other irregularities of hard rock over which the ice has just passed.

Absarokite-Shoshonite-Banakite Series: By J. P. IDDINGS. This is a study of a peculiar series of igneous rocks associated with the normal andesites and basalts of the Yellowstone National Park, but differing from them mineralogically and chemically. These rocks are arranged under the three groups named in the title, of which the first contains the least SiO_2 and the third the greatest amount. The author concludes that this is a series variable in two principal directions chemically: in the ratio of alkalies to silica, and also in the silica percentages. The variations of other chemical constituents are to some extent functions of these variables.

Distribution of Gold Deposits in Alaska: By

Geo. F. Becker. During the past summer the author was sent by the U. S. Geological Survey to investigate the gold resources of Alaska. This paper is a very brief resumé of the results. He finds nothing phenomenally rich, but that there are paying quantities of gold in several localities seems clear from his account.

In this number there is a new department, viz: Authors' Abstracts. Under this will be found abstracts of a variety of geological publications, including some of the new U. S. Geological Atlases.

AMERICAN JOURNAL OF SCIENCE.

The January number opens Volume I. of the Fourth Series, or Volume CLI. since the establishment of the Journal in 1818. The leading article is by W. M. Davis upon the quarries in the Lava Beds of Meriden, Conn. This locality exhibits with great distinctness at the present time the two lava beds composing the ridge at that point, and the fractures by which the beds are faulted. These igneous outflows in common with most of the others which characterize the Triassic of Connecticut are viewed as extrusive lava beds, once horizontal and continuous, but now tilted, dislocated and denuded. The present paper discusses in detail the present relations of the outflows, with a number of idealized illustrations showing their position with reference to the accompanying sandstone and shales. It is urged that the former may be used as well as the latter in the study of the stratigraphy. A second geological paper is by Stanton and Vaughan, and describes minutely, with a diagram, the Cretaceous section exposed in Mexico and New Mexico, near the Initial Monument of the Mexican boundary survey, three miles west of El Paso. G. W. Littlehales discusses, from a mathematical standpoint, the form of isolated submarine peaks with reference to their relation to the intervals at which deep-sea soundings should be taken in searching for probable shoals in the open ocean. E. H. Forbes gives an analysis of the epidote from Huntington, Mass., with a discussion of its optical properties and, further, their relation in general to the composition of the species. H. L. Wells and H. W. Foote describe a series of double fluorides of Cæsium and Zirconium; analyses of the salts $2\,Cs.ZrF_4$, also $CsF.ZrF_4.H_2O$ and $2\,CsF.3ZrF_4$ $2H_2O$ are given. Other chemical articles are by F. A. Gooch and A. W. Peirce on the iodometric determination of selenious and selenic acids, and by P. E. Browning on the interaction of chromic and arsenious acids. A. M. Mayer gives a note on the Analysis of Contrast-Colors by viewing, through a reflecting tube, a graded series of gray discs, or rings, on colored surfaces. This is based upon the fact, noted by Rood, that the mixing of black with certain colors simply darkens them, while with other colors the effect is to change the hues. A new form of cathetometer of simple construction is described by F. L. O. Wadsworth, with a series of figures and a half-tone plate showing the instrument in use. The novel feature is the employment of a light silvered mirror mounted on a vertical axis just in front of the objective. By means of this the comparison of the object to be measured with standard scale is readily made. It is shown that highly accurate results may be obtained with this instrument, while the cost is relatively very small. O. C. Marsh details some observations made of globular lightning from notes taken at the time of its occurrence at Southampton in July, 1878. The circumstances were such that this rare phenomenon could be more minutely and accurately observed than is often possible.

The concluding thirty pages of the number are devoted to abstracts of scientific papers, notices of books, etc., on a wide range of subjects.

SCHOOL OF MINES QUARTERLY, NOVEMBER.

The November number of the School of Mines Quarterly has recently appeared, J. F. Kemp taking the place of A. J. Moses as managing editor, as Dr. Moses is in Europe on a year's leave of absence. The table of contents contains the following: 'The Missouri River,' by George R. Morison; 'Temperature of Gases from Lead Furnaces' and 'Temperature of Lead Slags,' both by Malvern W. Iles; 'The Assay of Platinum,' by E. H. Miller; 'Lecture Notes on Rocks,' by J. F. Kemp; 'The Study of Architectural History at Columbia College,' by Wm. R. Ware. The first paper describes the peculiar features of the Missouri River and the difficulties met and surmounted in constructing and maintaining bridges across it. The author

is reputed to have built more bridges than any other living engineer, and presents an interesting account of his experiences. The next two give the results of a series of experimental determinations of the temperatures mentioned in the title. In the fourth paper the results are detailed of an extended series of experiments on a difficult subject and the final attainment of a feasible and a not too long method. The fifth paper, which will be a serial, contains the opening chapters of a manual on rocks for use without the microscope. The last paper emphasizes the importance of teaching architecture as an art, comparable with artists' as distinguished from engineers' or artisans' work. As outlining a future policy for our schools of architecture it has important bearings.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES, SECTION OF BIOLOGY.

THE following papers were presented on December 9th:

Prof. C. L. Bristol: 'The Classification of Nephelis in the United States.' The study of abundant material, collected from Maine to South Dakota, has shown that the color characters cannot be depended upon for specific determination. An examination of the metameral relations of this leech indicates that no more than a single species occurs in this country.

Prof. H. F. Osborn: 'Titanotheres of the American Museum of Natural History.' The complete skeleton of Titanotherium robustum is remarkable in possessing but twenty dorso-lumbar vertebræ, a number identical with that typical of the Artiodactyla, but entirely unique among Perissodactyla. It now appears probable that the development of horns in the Titanotheres became a purely sexual character, and that the genera Titanops, Marsh, and Brontops, Marsh, are founded respectively upon male and female individuals of Titanotherium robustum.

Dr. J. L. Wortman: 'The Expedition of 1895 of the American Museum of Natural History.' The expedition passed into the Unita beds of northeastern Utah, then between the eastern escarpment of the Unita range and the Green River into the Washakie Beds of southwestern Wyoming, the most important result geologically being that the Brown Park deposit is found to be of much later age than the Unita.

BASHFORD DEAN,
Rec. Sec'y, Biological Section.

SECTION OF GEOLOGY AND MINERALOGY.

THE Section of Geology and Mineralogy of the New York Academy of Sciences assembled for its regular monthly meeting Monday, December 16, 1895, Prof. J. J. Stevenson presiding.

The first paper was by Prof. H. P. Cushing, 'Notes on the Areal Geology of Glacier Bay, Alaska.' The paper will appear in full and with a geological map in Vol. 15 of the Transactions of the Academy, but the following is an abstract:

After an introduction which outlined the previous work in the region by Dr. H. F. Reid and the writer and the petrographical determination of the rocks that had been collected by them, and that had been studied by the late Dr. George H. Williams and the writer, a description of the general geology was given, based upon a geological map.

Mr. Cushing shows that the rocks present are argillites, limestone, quartz-diorite, diorite, crystalline schists and dikes of diabase. The argillites have a wide distribution around the eastern side of the Muir glacier basin, and also form the mountains adjacent to Muir Inlet. They present three main phases: First, very hard, fine grained argillo-siliceous beds, gray to brown in color, occasionally approaching quartzite in character. Second, blue and black, somewhat slaty rocks, nearly as hard as the first, and equally fine grained, but less siliceous, although containing only a slight amount of calcareous manner. Third, thin bands of black graphitic slates, with good slaty cleavage, and interstratified with the other two varieties. No fossils were found, although careful search was made.

The limestone is called the 'Glacier Bay Limestone.' It is dolomitic, and for the most part extremely pure, containing only a trace of insoluble matter. Fossils were rare and so damaged by metamorphism as to be unrecognizable. But in 1893 a fossil coral was brought

from the region by Prof. Stevenson, which had certainly been derived from this limestone. It was identified by Prof. H. S. Williams as a species of *Lonsdaleia*, and was regarded as demonstrative of the carboniferous age of the beds.

The quartz-diorite is a homogeneous rock, consisting of white plagioclase, with frequent thin prisms of hornblende, and occasional biotites and some quartz. A contact was found between it and the argillites which seemed clearly an irruptive one. Other contacts observed by Dr. Reid with the limestone also indicated contact metamorphism.

The diorite is a more basic rock than the quartz-diorite, and is found in the moraines. It has probably come from the mountains, which have yet proved inaccessible.

The crystalline schists embrace mica schists and actinolite schists and were obtained from erratic blocks.

The diabase dikes have all been intruded since the metamorphism of their wall rocks and are the latest rocks in the region. Mr. Cushing gives a detailed comparison of these rocks with other Alaskan sections, noting many parallel features and some contrasts. The paper concludes with a detailed petrographical description of the crystalline rocks.

The second paper of the evening was by Heinrich Ries, on 'The Geology of Orange County, New York.' Mr. Ries gave a resumé of the results obtained by him while in the field the past summer under Prof. James Hall, State Geologist, to whom the report will be made. The paper was extemporaneous and was not intended for publication. It was illustrated by numerous lantern views and geological sections.

The third paper was by Theodore G. White, on 'The Faunas of the Upper Ordovician Strata at Trenton Falls, New York.' Mr. White described the results of a visit to this, the typical locality of the Trenton formation, and of a detailed study of the faunas of each stratum of the limestones at Trenton Falls, and Poland, Oneida County, New York. The work was undertaken in connection with a doctorate thesis on the Trenton Faunas of the Lake Champlain Valley, which will be submitted in the spring to the Faculty of Columbia College. The faunal lists at Trenton Falls will be published in full in the Transactions of the Academy of current date.

By making use of conspicuous and constant layers as datum planes, the thickness of the beds in the Trenton Falls gorge was found to be 331 feet. On the same creek, three miles below Poland, underlying strata were found as follows:

Black River limestone, 11 feet 9 inches.

'Dove' limestone, 5 feet 1 inch.

Calciferous strata, 8 feet.

Various peculiar distortions of the beds in the Trenton Falls gorge was also shown and discussed.

The paper was illustrated by numerous lantern views from photographs.

The fourth paper of the evening by J. F. Kemp and T. G. White, 'Additional Notes on the Distribution and Petrography of the Trap Dikes in the Lake Champlain Region,' was postponed until the next meeting, on account of the lateness of the hour.

J. F. KEMP,
Secretary.

BIOLOGICAL SOCIETY OF WASHINGTON, 250TH MEETING, NOVEMBER 30.

THE first paper, *Some Fundamentals of Nomenclature*, by Dr. Edward L. Green, is printed in abstract in this journal.

Mr. Theo. Holm made some *Contributions to the Flora of the District of Columbia*, illustrating the same by specimens. Since the publication of the third list of additions to the flora many rare plants have been reported, some of which are new to the District. It was shown that the genus Panicum is exceedingly well represented in the local flora, and seven species were enumerated as not having been before reported. *Sporobolus vaginæflorus*, which was formerly known only from one locality, has now spread to several distant places and may be considered as rather common. Several rare Cyperaceæ were reported, among which *Kyllinga pumila* and *Cyperus aristatus* were new to the flora. The genus Polygala appears, like Panicum, to be widely distributed in the District, and *P. ambigua*, *P. incarnata* and *P. verticillata* were reported from several places. *Plantago Patagonica*,

var. *aristata*, had commenced to spread so as to become a weed in the eastern part of the District. After enumerating a number of similar plants rare in the District, the speaker made some brief remarks upon the morphology of some of these, *e. g.*, *Pogonia ophioglossoides*, *Orchis spectabilis*, *Smilax herbacea*, etc.

The evening was devoted to an address by by the President, Surgeon General George M. Sternberg, U. S. A., on the Practical Results of Bacteriological Researches.

F. A. LUCAS,
Secretary.

ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

AT the 241st meeting of the Society held December 17th, a paper on 'The Animistic Vampire in New England' was read by George R. Stetson. This superstition of ancient Babylonia, Chaldea and the far East by some mysterious survival, occult transmission or remarkable atavism, is prevalent in the scattered hamlets and more pretentious Villages of central Rhode Island. It is an extraordinary instance of a barbaric superstition out cropping in, and coexisting with a high general culture, and which is not so uncommon, if rarely so extremely aggravated, crude and painful.

The superstition is there unknown by its proper name. The local belief, however, precisely corresponds to the statement of the vampire superstition contained in Calmet's 'Traité sur les apparitions des esprits et sur les vampires ou les revenans de Hongrie, de Moravie, etc,' Paris, 1751, and as it now survives in eastern and western Europe.

It is, that a wasting disease is not a physical, but a spiritual ailment, obsession or visitation; that as long as the body of a dead relative of the person attacked has blood in its heart it is proof that an occult influence steals from it for death, and is at work draining the blood of the living into the heart of the dead and causing his rapid decline and death.

As in the middle age, the Rhode Island vampire is located, if, on opening the grave, the body is found to be of a rose color, the beard, hair or nails renewed and the veins and heart filled with blood.

The means taken for relief are also precisely those followed in parts of the Levant and elsewhere, viz: exhumation of the body and burning the heart and scattering its ashes to the winds. The persons indulging in this superstition in Rhode Island are not foreigners, but native born New Englanders. It is declared upon excellent authority to be prevalent in all the isolated districts of the southern parts of the State and that many instances of it can be found in the large centers of population.

As to its origin in Rhode Island there is no record; it is in all probability an exotic like ourselves, originating in the mythographic period of the Aryan and Semitic peoples.

No known precise parallel in the western Indian mythology has come to our knowledge. The Ojibwas and Cherokees have, however, something analagous.

Abundant evidence is at hand that the animistic vampire superstition still retains its hold in its original habitat; an illustration of the remarkable tenacity and continuity of a superstition through centuries of intellectual progress from a lower to a higher culture, and of the impotency of the latter to entirely eradicate from itself the traditional beliefs, customs, habits, observances and impressions of the former.

Mr. William Eleroy Curtis read a paper on the Regulation of the Social Evil in Japan, reviewing the legislation and imperial edicts that have appeared on that subject and describing the present method of confining prostitution to certain quarters of the cities and towns and making those who practice that profession practically prisoners under the constant surveillance of the police. The government of Japan prohibits any woman from following the business of a courtesan without the written consent of her parents, or her guardian, if she be an orphan, and requires her to make a contract for a term of years with the keeper of some hashi-zashiki; as the houses of prostitution are called. During this period she is not permitted to leave the limit of the Yoshiwara, as the quarter is designated, except on certain occasions which are enumerated in the law, or upon the expression of a desire to reform. When her contract is cancelled her license is surrendered, and she becomes a ticket-of-leave woman, subject to police surveillance until she

has demonstrated the sincerity of her intention to lead a different life. The patrons of the Yoshiwara are required to register their names, residences and occupations in books that are always accessible to the public and the police, and an account of their expenditures is accurately kept.

Mr. Curtis asserts that this system has been remarkably successful both from a sanitary and a moral point of view.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

THE 112th regular meeting was held December 5, 1895. Mr. Hubbard read a paper on 'Distribution of Certain Species of Mytilaspis.' He spoke of the unreliability of tradition and early records as a source of exact knowledge concerning the introduction and spread from one country to another of scale insects which are so easily transported and difficult of specific identification. He refered particularly to the published accounts of the introduction into Florida of *Mytilaspis gloveri* and *M. citricola*. The former is supposed to have been brought to Mandarin in 1838 by Mr. Robinson, on two trees obtained in New York from a ship which came from China, and the latter was said to have been brought to Florida some years later upon lemons from Bermuda. According to the speaker, both of these positive statements, hitherto unchallenged, are probably erroneous. The insect mentioned by Glover as having been brought from Bermuda is not a Mytilaspis, and *M. citricola* at that time had not yet reached Europe from the East. It certainly did not reach Florida much before 1880. *M. gloveri* is to-day the principal pest of the orange in the interior of Mexico, and it is probable that it was introduced with the orange into Florida and Mexico by the Spaniards at the end of the 16th or beginniug of the 17th century. Its appearance in 1838 was only the continuation of an epidemic of Coccid pests of the orange which is known to have overwhelmed the citrus plantations of Europe in the early part of the century, and to have spread westward sometime later to the Azores, Canaries, and finally to Bermuda. The speaker suggested that the obvious tendency to variation in form and thickness among the scales of Mytilaspis had produced in North America from an original tropical species *M. pomorum*, *M. citricola* and *M. gloveri*.

Dr. Stiles exhibited a Dermestes larva taken from a corpse 3 to 6 months after death. He referred to the statement by Mégnin in his 'La Faune des Cadavres,' that the period from burial of a corpse to its final dissolution may be divided into eight portions, each of these portions being characterized by the presence of a different series of insects. In regard to the manner in which insects gain access to a corpse, Mr. Hubbard said that with the Diptera the egg must be deposited on the outside of the coffin before burial, since he does not believe it possible for the young larva to make its way through the soil after burial. Dr. Stiles said that he did not agree with Mégnin in many of his conclusions, but considered the field a very interesting one for investigation by entomologists.

L. O. HOWARD,
Secretary.

[Abstract of report by D. W. Coquillett, Acting Secretary.]

ACADEMY OF SCIENCE, ST. LOUIS, DECEMBER 16, 1895.

THE Academy held its regular meeting at the Academy rooms with President Green in the chair and twenty-eight members and visitors present.

The committee to nominate officers for the ensuing year made report of following nominations:

President, Melvin L. Gray.
1st Vice-President, Edmund A. Engler.
2d Vice-President, Robert Moore.
Corresponding Secretary, Allerton S. Cushman.
Recording Secretary, Wm. Trelease.
Treasurer, Enno Sander.
Librarian, Gustav Hambach.
Directors, John Green, Adolph Herthel.
Curators, Julius Hurter, Herbert A. Wheeler, George R. Olshausen.

Prof. J. H. Kinealy presented his new instrument for testing the purity of air in buildings and gave an explanation of the method employed.

A. W. DOUGLAS,
Recording Secretary.